-/3

WW

Bought at Sydney Sale

Fig. 8.

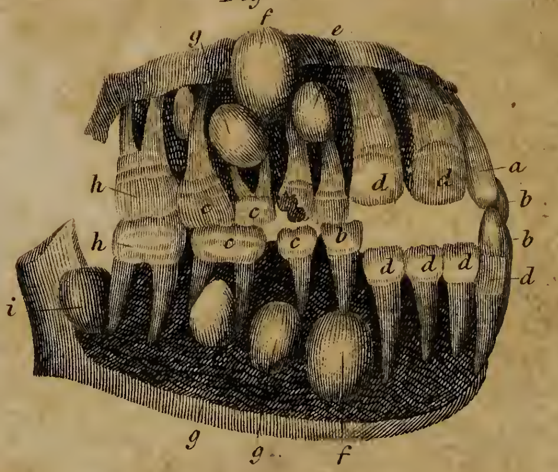


Fig. 8, represents portions of the upper and lower jaws of a child about seven or eight years old, the anterior plate of each jaw being removed, to shew the position of the temporary and permanent teeth, and the progress of ossification in the latter. The teeth marked *a*, *b*, and *c*, are temporary teeth; *a*, one of the lateral incisors of the upper jaw; *b*, *b*, *b*, the cuspidated teeth in both jaws; *c*, *c*, *c*, *c*, the temporary grinders; those marked, *d*, *e*, *f*, *g*, *h*, *i*, are the permanent teeth in different stages of ossification; *d*, *d*, *d*, *d*, *d*, *d*, the four incisors of the lower jaw, and two of those in the upper, which have just appeared through the gum; *e*, one of the lateral incisors of the upper jaw, not yet cut; *f*, *f*, the cuspidated teeth on one side, still imperfect; *g*, *g*, *g*, the bicuspides in the same imperfect state; *h*, *h*, the first grinders on the right side, nearly perfect, and at their proper height above the level of the gum; *i*, the middle grinder of the right side in the lower jaw still imperfect.

1822

J. G. Lightner No 18

Baltimore

J K m— vol 162—

THE
BALTIMORE
MEDICAL AND PHILOSOPHICAL
LYCÆUM,

BY NATHANIEL POTTER, M. D.
Professor of the theory and practice of Physic, in the
COLLEGE OF MEDICINE OF MARYLAND.

> " Non eram nescius, cum id opus agressus sum, fore ut hic noster labor in varias reprehensiones incurret.
>
> —— Etiamsi minus rem ipsam probent, aliquam tamen optimæ voluntatis meæ rationem habeant." QUINTILIAN.

Baltimore:
PUBLISHED BY GEORGE HILL,
And sold by him, at his book and stationary store, No. 104, *Market-street. Sold also by T. Dobson, J. & A.Y. Humphreys and E. Earle & Co. Philadelphia ; D. Longworth, T. & I. Swords, Collins & Perkins, and E. Sargeant, New-York ; Webster & Skinner, Albany ; E. Cotton, Boston ; Cushing & Appleton, Salem ; Thomas & Whipple, Newburyport ; Joseph Milligan, Georgetown ; Daniel Rapine, Washington City ; Somerville & Conrad, Petersburg. ; Wm. Prichard, Richmond ; Bonsal & Conrad, Norfolk ; J. T. Lawrence, Savannah ; and Jno. Hoff, Charleston, S. C.*

B. EDES, PRINTER.

DISTRICT OF MARYLAND, TO WIT:

Be it remembered, That on this tenth day of May, in the thirty-fifth year of the independence of the United States of America, George Hill, of the said district, hath deposited in this office the title of a book, the right whereof he claims as proprietor, in the words and figures following, viz.

" The Baltimore Medical and Philosophical Lycæum, by Nathaniel Potter, M D. Professor of the theory and practice of Physic, in the college of Medicine of Maryland.

" Non eram nescius, cum id opus agressus sum, fore ut hic noster labor in varias reprehensiones incurret.

—— Etiamsi minus rem ipsam probent, aliquam tamen optimæ voluntatis meæ rationem habeant." QUINTILIAN."

In conformity to the act of the congress of the United States, entitled, " An act for the encouragement of learning, by securing the copies of maps, charts and books to the authors and proprietors of such copies during the times therein mentioned," and also to the act entitled, " An act for the encouragement of learning by securing the copies of maps, charts and books to the authors and proprietors of such copies during the times therein mentioned" and extending the benefits thereof to the arts of designing, engraving and etching, historical and other paintings.

PHILIP MOORE,
Clk. Dis't. of Maryland.

THE

BALTIMORE

Medical and Philosophical Lycæum.

No. I....Vol. I.

FOR JANUARY, FEBRUARY AND MARCH....1811.

PROSPECTUS.

IN a science like that of medicine, where the wisest votary lives only to learn, it would, at first sight, seem superfluous to assign the motives for attempting to reflect light upon any one of its various departments. Bookmaking, however, has become a trade so common, that the world has been led to suspect most literary proposals, as projects to extract money. Under this impression, it becomes necessary, for those actuated by the purest motives, to cover themselves from the shafts of criticism and censure by the fashionable ægis, an apology.

None but a coincidence of circumstances peculiar to myself, could have impelled me to embark in so hazardous an enterprize, as the publication of a Medical and Philosophical Journal in this city. As it is problematical whether the subscription will ever be sufficient to encounter the expense, my efforts can be considered only as an experiment; and the possibility of a failure, must exonerate me from the imputation of pecuniary views. I am exquisitely sensible of the arduous duties the office of an editor imposes, and arrogate to myself no pretensions but an ardent zeal to subserve the interests of science and humanity.

When we reflect on the antiquity of the settlement of Maryland, the distinguished rank she has ever maintained in a civil and political view, the population of the city of Baltimore, and the felicity of her situation in the centre of the United States, it becomes a subject of equal admiration and regret, that in literature she has made no figure in the bright constellation of her sister states. In no department of science is this dearth of intelligence more to be deplored, than in that profession to which I have the honor, and perhaps the misfortune to belong. Notwithstanding the widely extended territory of the United States and the vast number of excellent literary institutions so young a country can boast, New-York and Philadelphia only have succeeded in establishing the periodical depositories of medical information. While far the greater portion of the Union affords nothing more permanent than the transitory ephemeris of a news-paper, the southern and middle states exhibit the most ample field for observation, and abound with distinguished philosophers and physicians, whose talents are rusting in obscurity for want of use. To such, it is to be presumed, an invitation only is wanting to excite a spirit of emulation, and to induce them to assert their just claims to literary pre-eminence.

The establishment of a Medical School in this city, the high temperature of the climate, and the shortness of the winters, which must ever preclude the possibility of a complete course of dissection further south, inspire the most sanguine expectation, that the rays of medical light scattered over our southern country, will finally converge in the city of Baltimore.

The relinquishment of the Medical Recorder, by Dr. Watkins, in consequence of his occupations at the Marine Hospital, and a desire of invigorating the spirit of inquiry it had excited, added to the solicitation of some of the most respectable of the faculty in the United States, suggested to me the necessity of a periodical work. At this moment the utility of such a work is greatly inhanced, by the exorbitant price of imported books. The physician who is determined to move in concert with the improvements of his profession, cannot accomplish his wishes without incuring a heavy expense; more especially when located at a distance from public libraries, only to be found in our most populous cities—Impressed with the importance of this fact, arrangements have been made for procuring from Europe the best periodical works, and a summary of their contents will be exhibited in the Lycæum, as succinctly and promptly as possible.

It will be proper to make some remarks on the more immediate objects of the work. The science of physic is the science of all nature. The physician who confines himself to the dead letter of medicine, cannot be enabled to take a liberal or expanded view of his profession. The sphere of such a miscellany must, therefore, be co-extensive with all the collateral branches of philosophy. Independent of the ordinary topics of medical discussion, there are others deeply interesting, and some of them peculiarly so to an American.

Dissections hold the first place among medical disquisitions. They not only teach us the causes and seats of diseases, but often lead us to the important determination how far the causes are general or local in their operation. Perhaps one of the most important questions in medicine is, whether the causes of diseases be local or general in the first instance: probably they are most of them primarily local. This important question must be determined by dissection and experiment.

Experiments conducted agreeably to the principles of sound logic, must ever interest the medical philosopher. There are many controverted points still left to visionary hypothesis and conjecture, that ought long to have been decided by experiment.

The intimate connection between chemistry and agriculture has hitherto, attracted too little attention, especially in

America. The nature of every soil must be ascertained through the medium of chemistry, for unless the husbandman understand the principle, he is an empyrick, and instead of certainty in his resutls, he is left to chance and the casualties of the elements.

Diseases are as necessarily the offspring of certain soils as the verdure by which they are clothed. The question how far certain diseases are attached to particular soils, as well as the means of obviating or destroying an inquinated atmosphere, are equally the business of the physician, the philosopher, and the citizen.

The strong affinity between chemistry and manufactures, is at this moment a subject of peculiar interest to every patriotic American. The art of *dying*, the most difficult attainment of our most important manufactures, depends essentially upon a knowledge of chemistry.

The animal, vegetable, and mineral kingdoms of our country, are all objects of the first importance to the manufacturer, the naturalist, and the physician. We have yet to learn how far our own country is capable of furnishing the various materials employed in medicine and the arts, for most of which we are still dependant on other nations. From the almost infinite variety of our soil and the progress made of late years in mineralogical researches, it is fair to conclude, that there lies hid in the bosom of our earth the richest abundance.

The variety of *Mineral Springs* that have lately been discovered in our country, especially in Maryland, and the almost irresistible popular current in their favour, render a chemical analysis indispensable. Until their properties shall have been perfectly ascertained, their most transcendent virtues must be used empirically.

The vegetable kingdom, viewed either in its connection with the arts generally or medicine, more particularly holds out the most inviting attractions. Many of our most energetic remedies are derived from it, and the vast unexplored wilds of our continent, probably offer to the botanist the richest repast now left upon the globe.

…

CONDITIONS.

The LYCÆUM will be printed quarterly, on fine paper and a neat type at Two Dollars per annum, one dollar to be paid on the delivery of the first number and half a dollar for each subsequent number.

☞ All Communications to be addressed to the Editor, or to Mr GEORGE HILL, Bookseller, Baltimore—(postage paid.)

WILLIAMSON

ON SCROFULA.

Baltimore, *February* 23*d*, 1811.

MY FRIEND,

ACCORDING to promise I now send you the first part of my essay on Scrofula, &c.

I know of no disease, to which the human system is subject that appears to be less understood and less successfully treated, than Scrofula. It has been my lot to meet with more Scrofulous patients in the course of the last four years than physicians generally do ; and it has been my good fortune to be successful in every case that has yet come under my care. I therefore feel myself bound to the faculty in particular, and to my fellow-citizens in general, to communicate publicly my information on the subject. I expected, in the course of the present year, to have published a small volume on this disease with some others that have excited much interest in the medical world, in consequence of their having been very fatal to the human species, but have been prevented from prosecuting these interesting pursuits, by having embarked in an extensive business that requires nearly the whole of my attention as well as my time. Owing to this disappointment, I have resolved to publish them, in their present imperfect state. I know of no better means of doing this than through the medium of some of our periodical publications, and have chosen the one to be conducted by you for that purpose. I hope with the exertion and aid of your friends, you will be able to give our city a more honorable stand, amongst those of our country, in a scientific view than it has hitherto held.

In treating of the Scrofula, I shall not enter into any speculations relative to its theory, but shall confine myself, principally, to practical remarks. After giving the cases and making some remarks relative to the treatment of the Scrofula, I shall notice the Phthisis Pulmonalis. Some of my remarks on this part of my subject may be considered speculative, they will, nevertheless, I flatter myself, be considered rational and interesting.

The first case of Scrofula that occurred in my practice was in May 1806.

The subject of this case was a negro boy aged eighteen years. The disease was seated in the glands, on each side of the neck. On one side the tumor was as large as a pigeon's egg, on the other as large as a hen's egg. The enlargement of the glands was discovered about three months previous to my being consulted. They were not ulcerated.

It were useless to give a history of the constitution of my patient, as medical authors of eminence have not unfrequently told us what constitutions are most subject to this formidable disease.

My patient's pulse was quick and irritable. He complained a little of nausea in the morning, but had ever, previously, been considered healthy, and his appetite was much impaired.

About the 27th of May, he commenced taking a mixture composed of cremor tartar, als. tart. and tartarized antimony. Medicines of this nature have been spoken of by Ontyd and other ingenious authors, as highly beneficial, previous to the administering of active emetics and cathartics, particularly in diseases of an obstinate nature. Whether the virtue of this preparation consists in its disolving or attenuating property in the primæ viæ, as contended by me, I will not pretend to say; but my practice warrants me in believing it a grand acquisition in the cure of many obstinate diseases, especially those of a febrile nature. I think it rational to suppose that when the system has been long affected with a disease, in which the primæ viæ participate, that their functions must be considerably impaired. That this is the case in fevers is very obvious, and that it is not in the Scrofula, I believe, no observant practitioner, who has paid any attention to this disease will be bold enough to assert.

It is only in fevers of a protracted nature, that I use this febrifuge as preparatory to more active medicines. I have used it in slow jaundice with a like good effect. In both of these diseases, it seems to prepare the way for the more active medicines that are to succeed. Emetics or cathartics which are administered after such a preparation have, from my observations, a much more salutary effect, in these lingering diseases, than those administered without. Perhaps it may be said, they then more effectually cleanse the primæ viæ. Should this be the case, our doctrine must be received without controversy. For whether the internal medicines, in which we most confide, communicate their influence directly to the stomach and from thence through the system by nervous influence, or pass on and act on the chylopoetic organs, or in any other way is equally immaterial, as the stomach is primarily interested; and the better it is prepared for the reception of a remedy, the action of which is to be on, or through it, the greater must be the success of that remedy.

After taking this mixture a few days, an emetic of tartarized antimony was given. The succeeding day, viz. the 1st of June, he took a purge of calomel and jalap; after which the chincona was given for a few days, to strengthen the tone of the stomach, and give fresh vigour to the general system. In a very few days he became quite voracious, and the bark was discontinued for tonics of another order.—In this stage of the disease, when the whole system is labouring under increased irritability, would not bark in large quantities or long continued be pernicious ?

On the 5th of June we began with the following medicine,

℞. Sulph. antim. præpt. ʒi.
Pv. Calomel ʒi.
Ex. cicutæ ℈i.
Sap. reent ʒſs.
Syr. simp. M. F. pil xxxvi.

One three times a day.

A strong volatile liniment was applied externally. His diet and drink, such as recommended by our best authors, in this disease. Every thing that seemed to disagree with the stomach, that was difficult to digest, or which would subject to acidity was guarded against. Diet easily digested, and at the same time as nutritious as possible, and such as makes healthy chyle, should be preferred. Are fluids as beneficial in this disease as solids of easy digestion ? Do not an excess of fluids enervate the stomach ; consequently the general system ?. They are recommended in inflammatory complaints as increasing the serum of the blood, thereby resolving its inflammatory action. If this is a correct theory, they must of course be pernicious in diseases of an atonic nature. I would by no means be supposed entirely opposed to the use of fluids in this disease, but think we should be cautious to admit none but those of a nutritious or stimulating nature, and then in moderation. His lodging was comfortable and dry ; his cloathing as clean as convenient. A humid impure atmosphere is pernicious to the animal system when in health ; in a state of disease it is more obviously so, and should be strictly guarded against in asthenic diseases, particularly those of a glandular affection. Cleanliness in cloathing, as well as that of the skin, &c. is very conducive to health. Perhaps keeping the skin clean has a greater effect in keeping up a healthy determination to the superficies than is generally imagined ; and cleanliness as regards the cloathing is certainly very necessary to support that of the skin.

June 16th.—The tumours were less, and the symptoms, in every respect flattering.

After the symptoms became very favourable, and the strumous tumours had considerably subsided, our patient imagined nothing was the matter, and for some days discontinued the medicine. When the appearance grew worse, and when I painted to him in the worst imaginable colours the nature of the disease, and what must inevitably be the consequence unless he could be cured and every disposition in the system to relapses completely eradicated ; he once more consented to have recourse to medical aid.

In diseases of long standing, it is often necessary to change our remedies frequently, and in none perhaps more so, than where the glandular system is principally concerned ; as these affections are generally very obstinate. It is an acknowledged fact that all medicines, in a greater or less degree, lose their effect from long use, or rather that the system becomes less susceptible to their operation.

22d.—We commenced with the murate of Barytes and extract of cicuta in conjunction.

I expected much from this composition ; my expectations were founded on the basis of great medical authority. The names of Crawford and Hufeland will ever be remembered with medical veneration ; the former of these worthy characters called the attention of the medical world to the use of the Barytes in the Scrofula, and the latter has been an able advocate for it in that disease. Such authority cannot insure success, but it should be sufficient to induce us to confide in what they say, and to give what they recommend from successful practice, and not vague theory, a fair trial.

* R. Muriat Barytes ʒi.
Extract cicut ℈ii.
Aqua font ℥ii.
Syrp. simp. Q. S.

---

* This Barytes was in a saline state.

We commenced with fifty drops three times a day.

Externally, a poultice of the crumbs of bread made with a strong solution of the muriat of amonia and extract of cicuta was applied three times a day. His neck was washed every morning with a strong infusion or decoction of bitter herbs and soap. He bathed once a day if convenient. Salt water was preferred.

June 30*th*.—The general symptoms were very favorable. Was at this period taking pills composed of sulp. antm. præpt. &c. which were stronger than the first.

August.—Tumors decreased and other symptoms were favorable. Was at this time taking the muriat of Barytes, &c.—had increased then to eighty drops, three times in twenty four hours. As an external application the lint. vol.

A short time before he was discharged cured, he conceited he was well, and irritated at my continuing to inforce the medicine and regimen—concluded one day to take a large dose and be done with it. He took ninety drops of the solution of the muriat of Barytes and cicuta at a dose. It occasioned considerable nausea, relaxed the whole system and was very near producing syncope. This alarmed him very much and he took special care afterwards not to take a larger dose than prescribed.

The effect this large dose had on the diseased glands was truly astonishing. It seemed to operate like a charm. The tremors were much softened by it, and discussed in a manner unprecedented. From this fact it appears that the muriat of Barytes, like other powerful medicines, has a much more benign effect, when given in as large a quantity as the system will bear, than when given in small doses. But a cautious physician should always exercise that caution which wisdom dictates. And in administering powerful medicines and those of a doubtful nature, begin with small doses, increasing gradually as symptoms, &c. may justify. By pursuing this plan he hazards nothing, provided he is using the medicine the disease indicates. It is also necessary after having discontinued a medicine for some time, on recommencing with it, to use the same precaution. Particular attention should be paid to the age, constitution, &c. Circumstances relative to the brain, lungs, stomach, &c. should be minutely examined. Some persons stomachs are so exceedingly irritable that scarcely any medicine can be given without exciting violent puking attended with alarming symptoms; others, again, are so acutely sensible as to be convulsed or paralysed on taking almost any medicine, and these disastrous affections are immediately conveyed to the brain or lungs through the nervous influence. Until we become acquainted with a patient's constitution, it it always necessary in giving active medicines, to act with extraordinary precaution. If we discover the person under our care to possess such a constitution as patients under similar circumstances, generally do, it is still necessary to extend our caution, lest some contingent circumstances may occur to thwart our intention or to render such treatment as we are then perscribing pernicious. By administering a medicine injudiciously, alarming symptoms may be excited; such as will deter the physician from a further use of it, or such as to make the patient positively object to its use. I doubt not but that circumstances of this nature are always pregnant with great evil, frequently destroying the reputation of the most invaluable medicines, also, that of *eminent* physicians, and rendering it impossible to cure the patient, whereas had a more judicous practice been observed, neither of these evils would have occured. Be cautious but not fearful, should be a ruling maxim with a physician. And while he is sedulously engaged in guarding against one extreme, in the practice of medicine, he should be as much so in avoiding the other. Whether he who prescribes too boldly, or he who does it too timidly, does the most mischief in society, is a question not easily solved. The fearful practioner trifles with his patient and suffers the disease to become so firmly fixed as never to be cured, when one with more energy vanquishes it, in its forming state. There are, doubtless, many remedies of inestimable value, which by being given in too small a quantity, have not the wished for effect, and are consequently abandoned as inefficacious, when had they been properly administered, they would have the the desired effect, and be justly esteemed.

I attribute the success of this case in a great measure, to the muriat of Barytes. How are we to account for its operation ? Is it necessary to have recourse to the irrational doctrine of specifics ? I hope not.

Scrofula is a disease of the system in general, and of the glands in particular ; and is, perhaps, owing to a morbid affection of the absorbents. Should this be the fact, are we to attribute the success of the Barytes, to any particular operation on or through the absorbents ? I presume not. From practice we are warranted in believing that there are many medicines that operate much more powerfully on the absorbent system than Barytes. Mercury and Digitalis both have, I believe this effect. Mercury was at one period prescribed in this disease from its supposed specific power on the diseased glands or on the absorbent system ; but except in combination with other medicines, it is now, I believe generally discarded, not only as useless but pernicious in this disease. And Digitalis, although its power on the absorbent system is truly wonderful, yet it would no doubt be very pernicious in Scrofula* I therefore think it rational to suppose, that the medicines which are salutary in this disease are so from their general tonic power ; and not from any specific property. It is possible, nay even probable that the muriat of Barytes and the muriat of lime, medicines at present the most celebrated in this disease, may operate on the absorbent system in an increased proportion to that of the general system. However, I believe their effect is not confined to the absorbents, but that their salutary power is first communicated to the stomach, adding tone to its muscles, enabling it to perform more completely its function, thence to the chylopoetic system, and so on until the general system is completely renovated.

---

*When I wrote the above lines I did not think that the Digitalis had ever been used in this disease ; but on looking over Beddoes on Scrofula I found the following sentence. " One day only I ordered a few drops of the Tincture of Digitalis, but on the occurrence of some unfavourable appearances, probably not owing to the Digitalis, I relinquished that medicine."

This conclusion is rendered at least rational, from the experienced utility of the corroborating medicines that are generally used with them; and also from the plan of practice pursued by those who have studied the disease.

The cold bath is certainly a very powerful remedy in many diseases of an atonic nature, and in none more so perhaps then the one in question. Its effects are general and not confined to any particular system.*

External applications, are, I conceive, of material service in this disease. The most active stimulants that can be employed without producing ulceration or corroding the skin, I suppose the most salutary. They stimulate the glands to an increased action and give additional energy to their absorbents. I also believe that washing the parts frequently may be of service. My object was not only to keep the parts clean, that the exhalants might perform their proper function, but also to keep up, for some time, a friction on the diseased glands, whilst washing with the bitter herbs, &c. which would also act as a local stimulus.

It is now (1808) near two years since I attended this patient, and there have been no returning symptoms of disease. There is a circumstance attending this disease, which, I conceive, should teach us a truth of the greatest magnitude, i. e. that no disease should be considered incurable. Although we may not have yet arrived at that highly to be desired point, yet, I believe it attainable, and that the day may come when the most formidable disease of whatever name may be cured. Although I contemplate with pleasure an improvement of this nature in the practice of medicine ; yet, I believe that day never will come when diseases of any name will not terminate fatally, even in the hands of men the most profoundly skilled in the healing art.

---

* Those who have read my essay on cold, published in the Medical Museum of Philadelphia, conducted by I. R. Coxe, M. D. will be easily enabled to apply my practice, in the present, instance to the principles there inculcated; and for those who have not, I will here merely observe, that I consider cold a sedative, and believe the cold bath to have a beneficial effect in Scrofulous cases from its accumulating the excitability and hence increasing the effect of subsequent stimulants.

It may be thought by some, that I have been too prolix in the minutiæ of the treatment of this disease. I am, however, confident that our frequent want of success in the treatment of obstinate diseases, is either owing, to our not paying sufficient attention to the minutiæ, or to a want of perseverance.

The following case which will conclude this part of my subject may be introduced in this communication, with more propriety than any subsequent one.

In 1807, I attended a patient, aged eight years, who was alarmingly affected with the Scrofula.

The general system was much emaciated. Seat of disease, the cervical glands &c. also, the parotids, with all the glands in these parts. Some of the diseased glands were as large as a pigeon's egg. The cluster of diseased glands, on each side of the neck, extending from the submaxillary and parotids, down to the cervical, looked not much unlike two thick bunches of grapes.

Nearly the same treatment was pursued in this case as in the preceding, except not being able for some time to procure the Barytes, the muriat of lime was used. I was afterward fortunate enough to procure the Barytes. From my experience in this case, as well as others, I am positive in the belief, that the Barytes is by far the most valuable medicine in Scrofula.

In this case our prescriptions were very imperfectly attended to, but they had nevertheless the wished for effect.

I have had other cases of Scrofula, where the disease has been slight, cured by the common tonics.

With respect,
I am yours, sincerely,
G. WILLIAMSON.

Nathaniel Potter, M. D

From the Philosophical Transactions.

ON THE STRUCTURE AND USES OF THE SPLEEN.

BY EVERARD HOME, ESQ. F. R. S.

MR. HOME while investigating the structure of the stomachs of different animals, observed, that in the fourth stomach of ruminating animals, in the single stomach of others, and even in the human stomach, there is a division, more or less evident, between the cardiac and pyloric portions; that while digestion is going on, these portions are seperated by means of a permanent or muscular contraction; that the food is prepared, softened, and dissolved in the cardiac portion and the chyle separated in the pyloric.

In some animals, as in the water-rat the separation between the cardiac and pyloric cavities is very remarkable, and their difference of structure and function abundantly evident; but, in all stomachs, this difference can be traced.

" The first instance in which this muscular contraction was observed in the human stomach was in a woman who died in consequence of being burnt. She had been unable to take much nournishment for several days previous to her death. The stomach was found empty, and was taken out of the body at a very early period after death. It was carefully inverted to expose its internal surface, and gently distended with air. The contraction was so permanent, that after the stomach had been kept in water for several days, in an inverted state, and at different times distended with air, the appearance was not altogether destroyed. Since that time, I have taken every opportunity of examining the human stomach recently after death, and find, that this contraction, in a greater or less degree, is very generally met with. That the food is dissolved in the cardiac portion of the human stomach, is proved by that part only being found digested after death: the instances of which are sufficiently numerous to require no addition being made to them.

" This could not take place, unless the solvent liquor was deposited there. Mr. Hunter goes so far as to say, in his paper on this subject, there are few dead bodies in which the stomach, at its great end, is not, in some degree, digested.— That the chyle is not formed there, and also, that it is completely formed before the food passes through the pylorus is proved by the result of some experiments of Mr. Hunters, made upon dogs, in the year 1760; and as they were instituted for a very different purpose, that of determining whether the gastric juice is acid or alk aline, the results were detailed without any possible bias.

" In all the dogs, the food was least dissolved, or even mixed toward the great end of the stomach, but became more and more so towards the pylorus; and just within the pylorus, it was mixed with a whitish fluid like cream which was also found in the duodenum.

" From the result of these experiments," continues Mr. Home, " as well as from the analogy of other animals, it is reasonable to believe, that the glands situated at the termination of the cuticular lining of the vœsophagus, which have been described, secrete the solvent liquor, which is occasionally poured on the food, so as to be intimately mixed with it before it is removed from the cardiac portion; and the muscular contraction retains it there till that takes place. Such contraction being occasionally required in the stomach, accounts for its being more or less bent upon itself, which renders it more readily divided into two portions, by the action of the muscular fibres at that part where the angle is formed."

We have thought it necessary to make these extracts from Mr. Home's papers on the scructure of the stomachs of different animals, in order that those of our readers who have not yet had an opportunity of consulting them, might the better understand the account we are to give of his observations on the structure and uses of the spleen. And for the like reason, we shall subjoin Mr. Home's general conclusion, drawn from the series of facts and observations detailed in those papers.

" The solvent liquor is secreted in glands of a somewhat similar structure in all animals, but much larger and more conspicuous in some than in others."—" These glands are always situated near the orifice of the cavity whose contents are exposed to their secretion. The viscid substance found on the intereal membrane of all the stomachs that were examined recently after death, is reduced to that state by a secretion from the whole surface of the stomach which coagulates albumen."

This appears to be proved by every part of the fourth cavity of the calf's stomach having the property of coagulating milk.

" This property, in the general secretion of the stomach, leads to an opinion, that the coagulation of fluid substance is necessary for their being acted on by the solvent liquors and a practical observation of the late Mr. Hunter, that weak stomachs can only digest solid food, is in confirmation of it.

" In converting animal and vegetable substances into chyle, the food is first intimately mixed with the general secretion of the stomach, and, after it has been acted on by them, the solvent liquor is poured upon it, by which the nutritious part is dissolved. This solution is afterwards conveyed into the pyloric portion where it is mixed with the secretions peculiar to that cavity, and converted into chyle." [See philosoph. transac. part 2d. 1807.

Believing, then, that the cardiac and pyloric portions of the stomach are separated from each other by a permanent or muscular contraction, and that they perform different and distinct functions during digestion. Mr. Home, in the paper now before us, endeavours to shew, that there is between the cardiac portion of the stomach, and the circulation of the blood, a communication through the medium of the spleen; and that the use of this organ is to carry off the fluids from the cardiac portion.

1st. He observes that the fluids are chiefly found in the cardiac portion, while the consistence of the pyloric are commonly of uniform constance. It seemed, therefore, probable, that the fluids beyond what are necessary for digestion are carried out of the stomach without ever reaching so far as the pylorus.

2dly. He made the following experiment. The pylorus of a dog was firmly secured by ligature, and five ounces of water, coloured with indigo, were injected into the stomach; at the end of half an hour, two ounces of nearly colourless fluid were rejected by vomiting. The dog was now killed. Though the pylorus was completely closed by the ligature, the pyloric portion of the stomach was empty and contracted. "The cardiac portion contained about two ounces of solid contents, enveloped in a gelatinous substance, and one ounce of water with little or no colour, the indigo being completely separated from it, and spread over the surface of the internal membrane." Two ounces of fluid had therefore escaped from the stomach in half an hour. No absorbents could be seen; but "the spleen was turgid, unusually large and its external surface very irregular. When cut into, small cells were every where met with, containing a watery fluid, and occupying a considerable portion of its surface."

These cells he finds to be the glands of Malpighi,and cortpuscles of Cuvier.

These cells, are only distinct, and seen to contain a fluid, when examined immediately after the stomach had received an unusual quantity of liquids.

The fluid escapes when the cells are punctured. The cells are membranous, and have numerous arterial branches and plexus of veins ramifying on their coats.

The trunk of the splenic vein compared with that of the artery, Mr. Home found to be in point of size as five to one.

Mr. Home repeated his first experiments, making use of an infusion of madder instead of a solution of indigo, in hopes that the colouring matter might be detected in the spleen. The result, however, did not correspond to his expectation.

At last, on the suggestion of Mr. Brande, he made use of rhubarb, which speedily passes into the blood, and tinges the urine very soon after being received into the stomach, and very small portions of which can be detected by means of the caustic alkali, used as a test.

" Five drops of tincture of rhubarb, added to three ounces of water, are found to strike an orange tint, when the test is added. By means of this test, the rhubarb was detected in the urine of different persons, in seventeen and twenty minutes after the tincture had been swallowed. These circumstances being ascertained, Mr. Home performed the following experiment.

" On November 17th, 1807, at 35 minutes past 11 o'clock, five drachms of a mixture of tincture of rhubarb and water, in the proportion of a drachm to an ounce, were injected into the stomach of a dog, whose pylorus was secured.— At 20 minutes past one, two ounces of fluid were brought up by vomiting; ten minutes afterwards, another ounce of the mixture was injected, as were nine drachms more at half past 4 o'clock.

" The two last portions were retained, and at 8 o'clock in the evening, the dog was killed.

" On examining the parts after death, the pylorus was found to be completely secured; the stomach contained about two ounces of fluid; none of the absorbent vessels passing from its great curvature were in a distended state, so as to be rendered visible. The spleen was turgid as in the former experiment, and the urinary bladder full of urine.

" The urine tested by the alkali, received a deeper tinge of rhubarb than the human urine, after rhubard had been taken three hours by the mouth, and in other respects resembled it.

" When the spleen was cut into, the cells were particularly large and distinct. A portion of it then was macerated in two drachms of water for ten minutes, in a glass vial. All the parts were exposed to the water, by its being divided in all directions. The water thus impregnated was strained off and tested by the alkali, and immediately the reddish brown colour was produced in the centre and no where else, but in less than a minute it began to diffuse itself, and extend over the whole. A similar portion of the liver was treated in the same way, and the alkali was added to the strained liquor, but no change took place whatever. In this experiment the rhubarb was detected in the juices of the spleen, as well as in the urine; and as there was no appearance of it in the liver, it could not have arrived there through the medium of the common absorbents, carrying it into the thoracic duct and afterwards into the circulation of the blood."

Mr. Home here concludes, observing, "the discovery of this fact I consider to be of sufficient importance to be announced to the society, that when it is thus made public, I may be at liberty more openly, and on a more extensive scale of experiment, to prosecute the inquiry."

………………

Dallam, on the use of Cotton, in Burns.

DEAR SIR,

I RECEIVED your favour of the 22d instant, and hasten to communicate to you the result of my inquiries and information respecting the accidental discovery of the beneficial effects produced by the application of *cotton* to *scalds and burns.* Should you think the facts stated worthy a place in the Lycæum, they are entirely at your disposal.

CASE I.

A child of Captain R. in this neighbourhood, aged five years, was standing alone before the kitchen fire when a large kettle of *boiling water* fell and was suddenly dashed over nearly its whole body. The affrighted mother who was sitting in the adjoining room picking and carding cotton, flew to her darling child's assistance, and having undressed it as quickly as possible, discovered it to be badly scalded. No medical aid being near; in the agitation and distress of her distracted mind she seized a large bundle of cotton and applied it over the whole of the scalded parts. Soon after the application, the tortured and screaming infant became perfectly quiet, and fell into a gentle and easy slumber. The cotton was suffered to remain on several hours, and when it was removed there was not the least appearance of inflammation remaining. This case was communicated to me by a person of veracity who saw the child soon after the accident and there can be no doubt of its correctness.

CASE II.

Soon after the termination of the preceding cure, a black child belonging to Mr. S. of this county (Harford) aged eighteen months, while playing on the hearth, fell into the fire and was badly burned. Its screams drew its mistress to the kitchen, and having just before heard of the successful experiment with the cotton, she immediately covered the parts with a thick boulster of the same. She informed me that it succeeded completely, and that she had been convinced of its efficacy, although she had before no faith in it.

CASE III.

About three months ago *my own little daughter*, was crawling unobserved near the hearth and overturned a tin cup of boiling water, which a servant had just before brought to me to make a lather for the purpose of shaving. The water (which was boiling) extended over the whole of one of her arms and scalded it exceedingly. Being present, I immediately bound up the effected parts with fine cotton. She ceased crying in a few minutes, and was well enough to ride to Havre-de-Grace with her mother the same day. When the cotton was removed the cuticle was scarcely discoloured.

In what manner does cotton act in lessening pain and preventing or removing inflammation from scalds and burns? Is it by excluding atmospheric air simply, or by some specific power? Whatever theory may be adopted to explain the facts, they must remain undoubted, and if a still greater number would add to their validity, I could state them, for I have witnessed the same effects from the same application in many other cases. I presume those I have detailed will be sufficient to encourage others to give the remedy a fair trial.

I am moreover of the opinion, that the application might be serviceable in other inflammations if properly applied.—The following case would seem to favour such an opinion.—A respectable lady of this county was seized with a violent pain in one of her jaws, which was supposed to have been rheumatic. The pain was excruciating and the inflammation and swelling great. The usual remedies had been applied for several days without affording the least relief. She was advised to try the external application of cotton to the inflamed part. It succeeded completely, and she assured me that in twelve hours after the application the inflammation and swelling intirely disappeared.

Accept, dear sir, of my best wishes for the success of the Lycæum, and believe me

Yours very sincerely,

WM. M. DALLAM.

Harford County, January 20th, 1811.

...

The simplicity of the application of cotton in obviating the action of fire upon the living fibre, and the want of success in the use of means much more plausible, left my mind in a state betwixt incredulity and approbation. Having sometimes failed in the treatment of inveterate burns by all the modes hitherto recommended, and having lately been disappointed in the effects to have been expected from the spiritus Terebenthinæ (the celebrated remedy of Doctor Kentish) where gongrene ensued notwithing its immediate application. I resolved on availing myself of the first occasion of testing the efficacy of the cotton. Only a few days had elapsed after the reception of Dr. Dallam's letter before a case in all respects adapted to my views presented itself.

On the evening of the 7th day of February, while I was visiting the lady of Mr. John Leigh of this city, the family was suddenly alarmed by the cries of William Wheatly, an adopted child of Mr. Leigh, aged thirteen months. He was immediately brought to Mrs. Leigh's room where I was sitting, and upon examination I found that boiling water had been accidentally poured down his back from the spout of a tea kettle. The water first came into contact with the cuticle between the scapulæ, opposite the second dorsal vertebra and descended along the spine and an inch laterally to the right and left till it reached the glutæi muscles. In ten minutes after the accident, a covering of finely carded cotton was laid over the whole affected surface and confined in close contact with the cuticle. As the cries of the child were expressive of the most excruciating pain, and the solicitude of the family for its repose great, I was questioned respecting the propriety of an anodyne? Having determined to rest the case solely upon the virtues of the cotton, I evaded the question and resolved to remain with the patient, and observe as minutely as possible the progress of the case. In about fifteen minutes after the application the child become less agitated, in half an hour ceased crying and a profoud sleep ensued in forty minutes from the accident. The child continued to sleep for about an hour, when he suddenly awoke, and after crying a minute or two became composed. The cotton was now removed and another boulster applied. No mark of increased action was apparent, and a tranquil sleep soon followed. The child remained in this state unmolested by pain or restlessness till about six o'clock the next morning, when it awoke, resumed its usual habits of playfulness and good humour, took its usual morning repast and discovered no symptoms of disease. Agreeably to my request the cotton remained on the scalded parts till I removed it at nine o'clock. It was with difficulty I could trace even a rubefacience although the appearances had been so threatning the evening before. There was however a small exception to this pleasing appearance. There was a space of about an inch below the place first touched by the water that was vesicated. As I had not observed the precise situation of the cotton before it was removed, I was at a loss to account for this limited affection; I was informed by one of the attendants that the cotton had slip ped down during the night, and that the vesicated part had remained bare. The application was renewed as well to the vesication as to the other parts, and continued till the morning of the ninth, when it was finally removed. The vesication gradually declined, and on the twelfth the cuticle peeled off without the least ulceration, or even a discharge." Although I cannot satifactorily account for the operation of the cotton, I will state an occurrence that may lead to an experiment to determine it hereafter. Although the heat of the affected parts was augmented previous to the application it was afterwards dimimished to a degree less than the ordinary temperature of the skin, for on the removal of the application the parts were cooled while the cotton was preternaturally heated. It was to have been expected that the preternatural heat of the skin would have been communicated to the cotton till an equilibrium would have been produced according to the usual laws of coloric; but the fact was otherwise, for, the cotton was heated in the same ratio that the skin was rendered cooler. If it were admissible to call to our aid the electric fluid so conveinent and fashionable in cases of difficulty, we might hazard an hypothesis that cotton might be an excellent conductor, and possibly obey laws essentially different from those observed by common electricity or the matter of heat. EDITOR.

……

Wright on the Malignant Fever of Elkridge Landing and its vicinity.

THE prevalence of epidemic diseases now rests for elucidation upon the assumed basis of atmospherical influence. To confer upon their history its merited interest, and give to it the most extended prospect of beneficial influence, a meteorological register is a necessary appendage; or to say the least, an important auxiliary. I regret that it is not in my power to give one in the present instance, circumstances having precluded the possibility of bestowing upon it the necessary attention.

I have met with no record in the history of the bilious diseases of our country prevailing in situations remote from populous cities, which for malignity of symptoms, affords a parallel, or will bear a competition with the epidemic which ravaged this district, for three months of the last autumn; except, perhaps, in circumscribed spots, peculiarly favoring the origin and local concentration of the prolific agent in the production of this disease. Elk Ridge Landing is situated at the head of tide water, on what is called the Ferry Branch of Petapsco. For a considerable distance above, the river winds in a narrow sheet between two extended hills of very great and nearly equal elevation, in many places so dammed up, or obstructed in its course, as to afford reservoirs for mills and other manufactories. At this place however and below it, the hills still preserving their altitude, have opened an extended space, presenting in the interval a low and level champain, now cultivated as meadows and grazing grounds.

If the presumption with regard to the active agent in the production of bilious disease be correct, it is not to be wondered that in situations like this, it should successfully exert its baneful influence. From an abundant source of miasmatic effluvia, it rises involved with the evaporatory process, and finds an equilibrium of specific gravity, below the elevation of the river's bounds, where reverberating from hill to hill, it is receiving daily addition and daily concentration. It has been remarked to me, as the result of observation, by some of the old and intelligent inhabitants of this district, that in seasons when the river was swelled by frequent freshes, the neighbourhood was less the seat of disease than under opposite circumstances. Where freshes are frequent, and repeated at short intervals, this opinion would appear founded on plausible inference, for every addition of pure water might reasonably be supposed to assist in the dilution of the miasma, or in carrying off its matrix. The late season however resulted reversely, and contradicted this conjecture. A few days antecedent to the first attacks of bilious fever, the river had been swoln by a fresh to an extent rarely occuring at a similar season. I was from home when it occurred, but a gentleman of respectability informed me, that it brought with it a very considerable and highly disagreeable smell, which induced him to suppose, that some one of the numerous dams had given way, and that the evolution of its collected filth, produced the stench, this was about the 26th of July. On the 1st of August, at 2 o'clock, I was called to attend a lady, at the landing, between sixty and seventy years of age, and found her labouring under bilious fever. She was in the act of vomiting when I entered her room; it was spontaneous and she was throwing up bile of a pale green colour. This bile came away in lumps or clots about the size of a pullet's egg, of the consistence of jelly, but possessing considerable tenacity. I administered warm water and ordered the immediate preparation of weak chamomile tea. It was brought before the emetic effort was over, and she drank freely of it, and by the time that effort ceased, she had discharged nearly a quart of bile: the last came away fluid. I now obtained a history of the attack which came on at one o'clock, with indistinct chill, accompanied with slight shivering or horror, and unattended by any local pain. The pulse after the vomiting, was quick, soft and moderately full, the habit of the patient robust, considering her age. The symptoms were now abated and she was tolerably easy, but complained of some heat. I gave for the present spt. nitr. and ant. vin. as a sudorific, and at night she took four antibilious pills. These had not operated well next morning early, and she then took jalap and chryst. tart. twenty-five grs. each. They operated briskly and freely. I left her at twelve much relieved, but at one was hastily called to her again. She had had, as before, a slight chill, and as on the preceding day was vomiting freely when I entered the room: she was discharging clots of bile of a similar appearance with those of the day before, with one exception, they were now of a dark green colour, and she complained of pain and burning in the stomach. The same course was observed as at first, the vomiting liberally encouraged. To me it was a subject of astonishment to witness the quantity of bile thrown up, a small chamber vessel was nearly filled with it. When the vomiting ceased she seemed considerably exhausted, and a little warm wine and water was given her. The day was extremely sultry, and the exertion of vomiting under febrile action had produced free perspiration; it was highly offensive. The house had been thrown open for air, and this smell diffused itself into the adjoining apartments, two or three persons indeed who came to the porch where I was sitting to enquire how she was, perceived it and asked from whence it proceeded. Her face had now assumed, and preserved even in fever a sallow-like complexion, but there was no apparent bilious effusion on the tunica abbuginea; she complained of fullness of the stomach. Without waiting for a perfect apyrexy, I prepared and administered a large dose of calomel and jalap; it operated well and had ceased at midnight; she slept afterwards and was easy next morning, skin cool, pulse slow and soft, her tongue had not been furred from the first, it was now white and moist. She began this morning small doses of bark and wine, gradually increasing as the hour of expected accession came on; at eleven o'clock five blisters were applied, to the ankles, wrists and one over the stomach; by twelve these were in action, half an hour after æther was combined with the bark and wine, and repeated in fifteen minutes. At one, without any previous indication, she was seized with chill instantly succeeded by cholera. The dark bile poured copiously from the stomach and bowels, it resembled coffee grounds and towards the last was mixed with blood. Nothing would check it and she died at three.

On the evening of the same day upon which this lady died, I was called to a man about a mile distant; when I entered the house, I was assailed by a smell which drove me instantly from it, and strongly impressed my mind with apprehensive forebodings. The smell proceeded from an evacuation; I called the family and requested that some sugar and vinegar might be burnt in the room. His wife then informed me that he had been attacked two days before with chill, &c. and on the preceding day had taken a purge which operated well, that he had not been confined except during the paroxysm, and on the morning of the present day had eaten a hearty breakfast, and walked about until two o'clock, when he was attacked. I entered the house and found him dying; he expired in half an hour. When I re-returned to the landing I was requested to visit a young man sixteen years of age, who had been attacked the evening before with chill and vomiting. The discharge resembled coffee grounds, delirium had supervened at midnight and still continued, it was of the low comatose kind. The pulse was quick and small, the countenance Hippocratic, and the complexion deep orange, of which the trunk and extremities equally partook. When much effort was made to rouse him it succeeded partially, he obstinately refused to take medicine, and a fixed spasm of the baccinator and orbicularis oris prevented its forcible introduction. His parents objected to any farther effort and he was trusted to nature; on the second day in the evening a profuse general perspiration broke out, it soaked through the bed and dripped upon the floor. He recovered rapidly.

From this period until the first of November, this hydra-headed monster brandished the besom of destruction over the whole neighbourhood; man, woman and child were equally the subjects of its universal ravage. Anxiety for the ill, grief for the dead, or the forcible expression of corporeal suffering and mental alarm, were legibly written upon every face, and hung upon every countenance a perpetual gloom.

The necessary limits of such a communication as the present will not admit a comprehensive view of its subject. I will give its form of attack, course and termination, with what appeared to me the most successful mode of treatment.

As to its character it had none, and the late season is the first, in which I ever witnessed the indiscriminate prevalence of the remitting and intermitting form. Hitherto as far as my observation of bilious fever extends, when it assumed in the commencement the remitting character, it has preserved it through the season; intermittents were rare, and vice versa. Of the comparative severity of their character also, I had formed an opinion founded upon specious principles, which the late season materially affected, viz. that the twenty-four hours calm afforded for repair and preparation, enabled us to guard against or ward off the expected storm; but this calm was deceitful and those efforts unavailing in the late squalls.

With the general pathology or distinguishing characteristics of bilious fever, we are all acquainted, I shall pass them in silence, and pourtray only the prominent features of the one whose history I am recording. In the early part of the season its attacks were generally of the remitting kind, towards the close of it, they assumed the opposite character. The chill which marked the first inroad of this disease was extremely indistinct, disclosed only by faint shivering, livor of the countenance, cold extremities and the sensation of a cool aura passing up and down the back. This was so much the case that the ignorant part of my patients very frequently contended that no chill had preceded the attack. There were however some exceptions, and in those cases the extremities were very cold, and felt like the appendage of a foreign body, the lips and nails were purple, the countenance livid or verging to black, the eyes dull and heavy, the respiration laborious accompanied with singultus, and a sensation of universal muscular rigidity, or as it was termed numbness. The stomach was generally pointed out as the principal seat of disease, not often a nausea, but more commonly a sensation of fulness, heat and burning attended; it was called by the old people a colic; the head sympathised with it, by pain generally of the frontal section, with a prominent, heavy, watery eye. The pulse in the chill was contracted and scarcely perceptible, in the reaction it was not full and round, but soft and frequent. Perspiration was partial and fetid, confined chiefly to the head and face, the decline of the fit was by slow and gradual exhaustion. The tongue in the first paroxysm, was white at the edges with a streak of discolored mucus in the middle, moist and not furred; in the second, still moist, covered with dirty slime resembling soft soap, the glands enlarged, and furred; if they lived through the third, covered with a dry brown crust, contracted and inflamed at the edges rendering articulation difficult. I have said the head sympathised with the stomach, this was to a very great degree; it came on generally in the second or third paroxysm, producing obstinate coma, low delirium, and not unfrequently apoplectic symptoms, it uniformly proved one of the most dangerous attendants, I saw but one case of active mania. There was at first no pain in the region of the liver, but it not unfrequently became sore to the touch as the disease proceeded., In a great majority of the cases which came under my notice, the system generally was in a very low state of torpor, the intestines particularly could be urged into action only by the most powerful medicines. Where evacuants acted freely substituting a sensation of emptiness and ease for the before existing fulness and burning of the stomach, where the tongue became clean and the system rose into free and general action under their operation, the patients recovered rapidly and perfectly. On the contrary, where the action of medicine was partial, or indeed where this was not the case but they acted well, if the foulness of the tongue, the fulness and heat of the stomach, the irregular action and general torpor of the system remained or increased, the case was doubtful and the termination generally fatal. Where this was the result the patients generally went off in the quiescence of chill, or sometimes lingered a little longer in the process of mortification. I saw no case of gradual exhaustion from supervening typhus. In either event of recovery or death, they were restored speedily or their sufferings soon terminated.

TREATMENT.

Emetics.—In the generality of cases in which spontaneous vomiting came on early, it proved a favorable symptom. It was a nisus naturæ which pointed out the demand of the system; it was also an argument of the stomach's sensibility. To encourage the discharge I was at first in the habit of using warm water and chamomile tea, afterwards with better effect large draughts of a weak watery solution of the antimoniated tartarite of potash. Frequently however I found it necessary to give the emetic draught in full and free doses; the event was always satisfactory. I treated no case in which I regretted the use of this highly beneficial preface to the evacuant plan in bilious fever. Although the natural enixus alone has not been frequently effectual, it nevertheless appears to point out the proper mode, and instead of increasing that debility so formidable to some, nature relieved by evacuation from a state of oppression, will quickly rise into more healthy and vigorous action. I have somewhere seen an opinion advanced that bile becomes attached to the villous coat of the stomach and intestines like dirt to the skin; the difficulty of dislodging it, the solution of the disease by repeated and active evacuants only, and the very considerable quantity of inspissated tenacious bile always following their effectual exhibition, seem in some measure to countenance such an opinion. However this may be, I found the shock given to the system in vomiting essentially serviceable in rousing it from its general and dangerous torpor, in pouring out by compression and agitation the retained bile of the choledochus, and in relieving the stomach of its load. In one case I gave an active emetic to a man apparently dying, and with success. There may be however, and no doubt are certain states in bilious fever which forbid the exhibition of emetics; such as particular local affections of the breast and head, and absolute prostration from extreme exhaustion: but these are rare. I followed the emetics immediately by, Cathartics.—These were given as soon as the stomach was sufficiently quiet to retain them. For some time I used calomel and jalap in common doses, proportioned as near as possible to the habit of the patient; this did not succeed agreeable to my wishes; it was too slow and partial in its operation, and my patients were not generally relieved by it; for this I could afterwards account, I did not give it in the doses which I eventually found necessary; and which in truth I had no idea the system would bear. The next form of purge I used was jalap, crem. tart. and tart. emet.; this last was so combined as to sicken. With the operation of this I was much pleased, it seemed by nauseating to open the ducts and free the liver, and by relaxing the system to facilitate the transit from the bowels. It also produced a determination to the surface. This however I laid aside as not sufficiently powerful and too bulky, and returned once more to the calomel and jalap. I now gave sixteen and twenty. On the tenth of September I was called to a young lady twenty years of age, of a good constitution but bilious habit; I found her vomiting a fluid of the colour of a strong solution of indigo. The emetic draught tolerably strong was given and operated actively. After the vomiting had ceased she complained of pain in the head, breast and stomach; her pulse was full and springy. I took away twelve ounces of blood; this was the first blood I had shed in this fever. The pulse was reduced, and the head and breast relieved, but the burning at the stomach continued. She took a solution of carbonate of potash, and soon after sixteen grs. calomel and twenty of jalap in six pills. They shewed no disposition to operate by next morning, she then took thirty and thirty, and to support the operation I prepared six pills calomel each six grs. and left them, with orders that one should be taken every three hours until a free operation should be produced. She took them all in twenty-four hours. At night her pulse had risen under the operation, and I took away more blood. A solution of phosphate of soda was given in small doses through the night; she was much better next day, and soon recovered. The same patient relapsed three times in the course of the fall, was highly bilious each time, was treated always pretty nearly in the same way, and with success. The calomel each time affected her mouth; she took in the aggregate 180 grs. This plan of purging I afterwards pursued with the most pleasing result: except when circumstances absolutely forbid it. Thirty or forty were my usual dose, this was worked off with calomel pills, which frequently affected the mouth, and always with benefit. A respectable physician in the neighbourhood informed me that his medicines succeeded best where his patients fainted under their operation. When purgatives were commenced, I found it always best to continue or keep up their action by the introduction of others upon the decline of the preceding ones; four, five, six or more, were sometimes necessary in proportion to the ease or difficulty of dislodging the bile, the powers of the patient and the relief afforded the system. In forming an estimate on this head, the tongue was always a useful and I believe invariably correct oracle; I mean by its appearance. It uniformly became clean and assumed a healthy look, when the stomach and bowels were sufficiently unloaded, and not till then. The evacuant plan however was necessarily moderated in particular cases, and governed by existing circumstances. Some of my patients required a more delicate treatment; I will insert an example. On the eighth of October, I was called to a woman about thirty, of delicate habit. She had passed through two paroxysms of chill and fever of the remitting kind; she had been bled and taken a purge which operated well, but produced no relief. When I saw her about eight o'clock, A. M. she appeared extremely languid, with weak frequent pulse. Her stomach was very irritable, she was throwing up at intervals of only a few minutes the coffee-coloured fluid, and complained of burning or as she termed it knawing at the stomach; her respiration was anxious, accompanied with singultus. The period of attack on the two preceding days had been twelve o'clock, and another paroxysm I apprehended would prove fatal. She took a solution of carbonat. potash with essen. menth. pip.

and laudanum, to quiet the stomach; at eleven, P. M. hot cataplasms of mustard were applied to the feet so as to cover them entirely, and the same application was made to the stomach, arms and hands. The compound saline mixture checked the vomiting, and the chill did not come on. At two o'clock, the cataplasms were removed and the mixture discontinued. She now commenced magnesia in large doses, and continued them through the night; they operated smartly, and every operation diminished the burning, and took off the determination to the lungs. The stools resembled molasses or tar, and were very fetid. On the next morning, the magnesia was discontinued and very strong chamomile tea given; the sinapisms were again applied at eleven; she again escaped chill and returned at two to the magnesia. By pursuing this course for one more day, she was freed from danger. Sulph. sod. and tartarized antimony combined, composed another form of carthartic which I used in many cases of this fever. I used them generally in large doses for three or four mornings successively; where I wished to evacuate rapidly, they were particularly useful appearing to cool while they emptied the system.

BLEEDING.—It was not until about the first of October that I met with many cases which required, or would safely admit the use of the lancet. They had now assumed more of the intermitting fever, and were more generally accompanied with active febrile exacertation. I used it however in every instance when the powers of the system were not too sensibly depressed, in which cases I found emetics most useful in rousing those powers into action by unloading the stomach. In the first stages of the disease of this month, it was very frequently admissible, and in moderate quantity I found it a powerful remedy against all the symptoms. It is the neatest, speediest, and certainly a very safe method of moderating action, as we have it so completely under our controul. It takes off that troublesome determination to the head, and used in the exacerbation more certainly cuts short the paroxysm than any other remedy. The observation of writers who class bilious fevers among pyrexial ashenia, that the simple intermittent is by the depleting plan of cure, frequently brought into the continued form of typhus nervosa, I apprehend to be incorrect. Injudicious depletion may sometimes be a cause, but for the most part I am inclined to believe that a neglect of the depleting plan in the commencement lays the foundation for the typhoid character, and that the irregular, forcible and uncontrolled action of the vessels is what shatters the system and protracts its powers. I am aware that with many respectable authors the remote causes of this disease are certain sedative powers applied to the system inducing or increasing a state of debility is held the proximate cause. The predisposing causes are esteemed a state of constitutional weakness, previous disease, evacuations of every kind, &c. &c. This opinion has been much advocated, and seems to have been taken up more upon the ipse dixit of writers, than from observation. The varieties of situation have been left out of the picture, and it seems to have been forgotten, that most of those opinions have been given to the world, by writers of some of the warmest climates of Europe. One of those writers, Dr. Dickenson, of the Edinburgh Medical College, who has taken great pains to collect into one view, the phenomena, causes remote and proximate, and modus medendi in fevers, has gone so far as to observe that evacuations by purging in intermittent and similar fevers were extremely injurious, that bleeding was certain destruction, and that even a dose of nitre had in some instances almost irretrievably sunk the patient. In the course of his enquiries he goes on to blame the timidity of those who had hesitated to administer wine, bark and opium in the hotest stages of intermittent paroxysms. I can only observe, if this be correct practice the bilious diseases he is describing must be essentially variant from those of our own country. It was not always under those circumstances alone, where I was sure I was safe in bleeding, that I ventured to perform the operation. I met with several cases where the decision was delicate, yet I hazarded the practice. These were cases in which I did not see the patient until two or three days had elapsed, and the disease had ravaged unopposed: here there was much depression of the system accompanied with burning and pain of the stomach. In the first case where the experiment was tried, a large dose of calomel and jalap had been given, followed by ol. ricini and injections which produced no operation. On an attentive examination, the tongue was dry and inflamed, the serous vessels of the eye contained red blood, and the bilious hue of the surface was interspersed here and there with inflammatory streaks; the respiration was laborious and the head pained. Bleeding unlocked the system, medicine acted well after, and the patient recovered. In similar cases afterwards I did not hesitate, and I had much reason to be pleased with the practice. The pulse in these cases was small, but rose upon bleeding. I have said in the early part of this history, that coma and low delirium were frequent and dangerous attendants upon those fevers. A physician who attended this district with me informed me that every case he had met with proved fatal. About the tenth of October, I was called to a young man of spare habit, who had been attacked with bilious fever the day before. He was lying on a bed with his clothes on, having been walking about until ten, A. M. when he was again attacked with chill. As the fever formed, delirium came on. He was now at two, highly comatose with indistinct muttering, and no efforts would rouse him in the smallest degree. The skin was hot and dry, the pulse beating one hundred and fifty and small. A large tub of warm water was prepared into which he was placed, and a blanket wrapped round him so as to confine the vapour. I then drew from the arm slowly, three pints of blood. While bleeding, partial perspiration would frequently break out, but cease again in a short time; no alteration was produced. I staid with him three hours, during which he was kept in the water, to which additions of warmer water were made as fast as it cooled. The symptoms continued the same, and before I left him I again opened the vein and took away a pint more of blood. There were still no prospects of returning reason, and I left him with directions, that he should be kept in the bath three hours longer if necessary, and if not then relieved to be taken out, wiped dry, put to bed and hot mustard cataplasms applied immediately to his feet. I returned next morning and found him sitting up in bed. He had recovered his faculties before the period of his continuance in the bath expired. He soon got well, but his intellects were partially disordered for three or four days.

BARK AND OTHER TONICS.—Of the bark I made little use, if given before proper evacuations had been procured it aggravated the symptoms and increased the disease, and when those evacuations had taken place it was superfluous; for the system rose rapidly into healthy action. In most instances I believe the patient was dead or out of danger within ten days from the attack, the fatal remitting cases from forty-eight hours to five days, the intermittent protracted a day or two longer. In some few exceptions to this remark, a quotidian or tertian remained after the bile appeared evacuated. In those the bark was of no service, confining the patient to bed and procuring free perspiration by active sudorifics previous to, and supporting it at the time of expected accession, was the most successful in breaking the habit or chain of periodical chill or ague. This plan succeeded with me when bark, musk, blisters, camphor, opium, and æther were entirely useless. The best form of sudorific which I used was a table-spoonful of good emetic wine in a pint of Madeira: a wine glass full of it given warm every one or two hours so as to meet the fit. I used also in some instances Dover's Powder, and occasionally nauseating doses of emetic tartar and ant. powder. In some other instances where, in getting clear of the bile, an alarming state of debility was induced and chill apprehended, blisters to the wrists and sinapisms to the feet so as to be in action at the time of expected periodical accession, averted the danger most effectually. In addition to these, good porter was of service if the stomach would bear it: and after the apprehended period was over, chamomile and columbo were the best corroborants. How far does the manifest superiority of external stimulants over internal toncis in keeping of chill, favor Cullen's theory of cuticular spasm in its formation?

MISCELLANEOUS REMARKS.

Never before, I suppose, did a disease of this kind so generally pervade a neighbourhood or district, as did the late epidemic here. I remember but two or three persons within the supposed range of this airy-demon, who entirely escaped. Whether it was in any instance communicated by contagion, I should hesitate to say; there were a few cases which appeared to favor such an opinion; but as a certain degree of predisposition from improper quantity or quality of bile seems necessary to the constitution of the disease, it must remain a subject of doubtful speculation. One occurrence, which however proves nothing on this head, very generally took place; whenever this visitor entered a family large or small, it scarcely ever left it until every member of every description, had been subjected to the ordeal; and in families composed of fourteen or fifteen persons, I had frequently at the same time twelve or thirteen patients. On entering houses where this disease was raging, I was frequently attacked almost immediately with head-ache, nausea, and increased frequency of pulse. A removal into the open air, and gentle exercise on horseback would carry it off. One case which occurred to me this fall I will give, and leave without comment with regard to probable contagion. I had called to visit a woman whom I had attended some short time before in childbed; she had that morning, for the first time since delivery, left her room, and was sitting in good spirits in an outer apartment where I was conversing with her. A labouring man of the family at this moment entered in a paroxysm of bilious fever and laid down upon a chest; he was in a free perspiration and it was highly offensive. The woman complained very soon of giddiness and nausea and retired to her room, here vomiting soon ensued. The man died that night, and the woman a few days after. Sex, age, habit, constitution or peculiarity and incidental variety of situation, produced but little modification of this disease. The robust were more violently attacked, the symptoms more severe, the course less protracted, and the termination more frequently and more rapidly fatal, than in mediate situations. New comers into the neighbourhood, were greater sufferers than the long settled inhabitants. Blacks died less frequently in proportion than the whites; many of them however were roughly handled, and I observed in them a remarkable prevalence of languid or suffocated excitement. In the generality of those cases, the pulse was even in fever, very small and feeble, and I met with four or five in which, from the very first attack, there was no pulse perceptible at the wrists; they were dull and listless, much disposed to sleep or rather stupor, unwilling to move or speak, slightly delirious, and complained of no pain. Emetics active and repeated, and followed by brisk cathartics relieved them. In the course of the season, I saw but few cases of jaundice or the complete orange tinge: the complexion more generally resembled dirty tallow, and the capillary vessels seemed filled with a cream-coloured fluid. The tunica albuginea was frequently not at all discoloured. Petechiæ were common, and appeared generally in the decline of the second paroxysm. I met with one case where the powers of the system had weathered the storm without the aid of medicine. Here nature had produced a crisis by those local dispositions denominated carbuncles; there were twenty-four of them, containing a fluid resembling extravasated blood, or tar. The attack of this disease was in a great many instances preceded for a day or two by a constant and considerable burning in the palms of the hands. I had about the middle of September, this sensation myself, accompanied with acute pain of the head. I took away at once two pints of blood, and immediately after swallowed a cathartic; in the act of swallowing this dose, some of it passed into the trachea, and I was instantly seized with a convulsive cough; inspiration was prevented by the violent effort to throw it off, and I was very near perishing under the suspension. In this convulsive action I felt a sudden lancinating pain, with sensation of cracking or tearing in the liver, and, in fifteen minutes, operation commenced. This was at ten on Saturday, the operation continued till twelve on Sunday, and the quantity of bile discharged was truly astonishing; it came away like thick oil, of a dark green colour, and after a few evacuations totally unmixed with fæces. By living low, seldom eating dinner and then chiefly vegetables, abstaining from liquid stimulus, except occasionally a glass of wine, and avoiding other causes of increased excitement, I escaped for the rest of the season.

The commencement of this epidemic was with the month of August, it was most prevalent and most severe about the middle of September, and declined with October. Relapses were frequent, but generally mild.

T. H. WRIGHT.

……

Hopkins's Case of a Shot in the Trachea.

Anne-Arundel County, February 4th, 1811.

SIR,

I HEREWITH transmit the history of a case occasioned by the introduction of a *shot* into the *trachea*, which from its singularity and the extraordinary mode adopted for its relief appears to me worthy of record.

Perhaps there are no cases that occasion more distress and anxiety than those in which extraneous substances are intercepted in the pharynx or larynx, and none, therefore, for which we should be more studious to devise prompt methods of relief. The following are the particulars of the case to which I allude.

" A young lady of beauty and sprightliness espying a middle sized shot upon the floor, took it up, put it into her mouth and was gently chewing it while at the tea-table with several other young ladies. While one of them was relating a laughable circumstance that excited a general and sudden burst of laughter, the shot passed the epiglottes and lodged in the trachea. Violent coughing immediately succeeded to the accident and harassed the patient almost incessantly. Four physicians of the greatest medical repute were immediately sent for, who attended her for several weeks, during which time they made use of a variety of means to relieve the complaint or mitigate its symptoms; such as repeated bleedings, oleaginous demuleent and anodyne medicine, which in some measure appeased the cough, but afforded no prospect of effectual relief. In this state of the case the patient was abandoned by the gentlemen of the faculty: her deplorable situation, however, excited to an unusual degree, the sensibility and ingenuity of her mother, who, for good sense and ingenious device was scarcely equalled by any woman in the county. She put her invention on the rack to devise some scheme of relief more effectual than those adopted by the physicians. She recollected to have heard that the anatomy of a hog very nearly resembled that of the human species, and this determined her to have a hog killed and brought to her. She began a dissection and proceeded slowly from the top of the larynx down to the lungs, attentively observing the nature and use of the epilottis and thence conceived it practicable to propose some scheme to enable the shot to find its way out of the trachea. Here it is necessary to remark, that during the violent fits of coughing, the patient would often remark, that she felt the shot high up in her throat, as she expressed it. These circumstances having been revolved in the mind of the mother, she come to the resolution, that she would continue at the back of her daughter, and wait for a violent fit of coughing, and throw her with force out of bed upon her hands, supposing it probable that the fright and force added to the weight of the shot, might eject it from the glottis. The success of the experiment justified her conjecture, for during a violent fit of coughing, the mother (without having imparted the scheme to the daughter) suddenly threw her out, as she was bending over the side of the bed. She fell upon her hands, when, to her inexpressible satisfaction, she heard the shot roll upon the floor. From this time the patient gradually recovered, and enjoyed a tolerable good state of health for many years. She afterwards married, and had several children, but was subject to repeated attacks of hœmoptœ, and ultimately died of phthisis, palmonalis.

It is probable that the shock given to the lungs, by the long continued irritation of the shot, perhaps, increased by the poisonous quality of the lead, laid the predisposition to the disease ; and the roughness of the shot, from having been previously chewed, probably added to its irritating effects.

Although the result of the above case, does not redound much to the honour of the scientific gentlemen who attended this case, some useful lesssons may be drawn from it, and therefore it becomes necessary to record it. It may perhaps, tend to rouse physicians to a more vigorous exertion, and excite them to indefatigable industry in inventing ingenious modes of cure, whereby they may supersede the necessity of quacks and ignorant pretenders, stepping in during our moments of listlessness and inattention ; for by our aversion to mental exertion we often give them occasion to triumph over us to our shame and confusion. For these reasons it behoves us in this, our enlightened day, by assiduity and attention, to exhaust invention itself, for the utmost attainment of all the art it is capable of, that we may retain our excellent art among ourselves. When this shall be the case, we may predict without the spirit of prophesy, that the good sense of the community will put down quackery, and discountenace the use of nostrums, and the sale of Patent Medicines.

With the best wishes,

I am yours respectfully,

RICHARD HOPKINS,

Editor of the Baltimore Medical Lycæum.

From the Edinburgh Medical and Surgical Journal.

Report on a Memoir of Drs. Gall and Spurzheim, relative to the Anatomy of the Brain. By M. M. Tenon, Portal, Sabatier, Pinel, and Cuvier, presented to, and adopted by the Class of Mathematical and Physical Sciences of the National Institute.

THE physical class appointed M. M. Tenon, Potal, Sabatier, Pinel and Cuvier, to report upon a memoir, entitled, Inquiries into the nervous system in general, and the brain in particular, by Drs. Gall and Spurzheim.

Your committee must confess, that they had at first, some doubts about undertaking its examination.

The class has always adhered to a very wise rule of giving no opinion upon works already submitted in print to the great tribunal of the public ; it might be supposed, that the anatomical doctrine of Dr. Gall has obtained, by means of the lectures which he has delivered in the principal cities of Europe, and by means of the numerous extracts from them, published by his pupils, a degree of publicity almost equivalent to that of authentic publication.

Besides, this anatomical exposition of the nervous system is generally supposed to be intimately connected with, and, in fact, is connected to a certain degree, by its author, with a physiological doctrine which he teaches, concerning the special functions of the different parts of the cerebral organ, a doctrine which in no way comes under the cognizance of the class, since it ultimately depends upon observations relative to the moral and intellectual disposition of individuals, which certainly are not within the sphere of any academy of sciences.

Such were the motives which at first deterred us ; but others soon occurred which counterbalanced them.

Of all that has been written from the lectures of Dr. Gall, his opinions upon the anatomy of the brain have been announced with most confidence, and, at the same time, explained, with least detail and perspicuity. Besides, he does not acknowledge entirely any of the publications of his scholars ; and consequently none of them enables the public to judge of his ideas, or supersedes the necessity of referring to the memoir presented to you. Lastly, he has taken the greatest care, in this paper, to omit entirely those assertions which have rendered his name popular, by becoming the subject of warm discussion, among people of all ranks, and he has confined himself strictly to his anatomical observations. Whatever, therefore, your judgment may be, no conclusion can be drawn from it, concerning a doctrine which has only a very distant relation to anatomy.

The consideration of the importance of the functions of the nervous system, and of the ignorance which still exists, concerning several points of its structure, notwithstanding the numerous examinations to which it has been subjected, superadded to these motives fixed our determination. Whoever flatters himself that he is able to throw any light on a subject so interesting and so obscure, is, in fact, entitled to be heard with attention, by a body such as ours ; and we would be wanting in our first duty, if our examinations were not conducted with the greatest attention and strictest impartiality.

Forgetting, therefore, entirely what has been said and written for and against Dr. Gall, in public, in journals, or in pamphlets, not even confining ourselves solely to his memoir, which has not appeared to us to be written with all the order and perspicuity to be desired, we invited him, as well as Dr. Spurzheim, to our conferences. They readily dissected the brain in our presence, and we dissected it before them ; we afterwards re-examined, by ourselves, the observations they communicated to us ; lastly, we endeavoured for a time to adopt their manner of viewing the subject, and to give a clear and precise abstract of it, which we submitted to them, that they might be satisfied whether we rightly comprehended their ideas.

After taking all these precautions, we endeavoured to ascertain what was new or just in their ideas, and in the consequences which the authors deduced from them.

Experience very soon showed the brain to be the material instrument of the mind, and the essential organ of animal life, as well as that the whole nervous system performs a very considerable part in the functions of animal life. It is, therefore, not astonishing that physicians, anatomists, and phylosophers, in all ages, have investigated, with equal ardour, an organ of so much importance. It is with its study, that the history of anatomy commences and terminates. Democritus and Anaxgoras dissected the brain almost 3000 years ago, and Haller and Vicqd'Azyr, and twenty living anatomists, have dissected it in our own times ; but strange to tell, there are not one of them who has not left discoveries to be made by his successors.

Undoubtedly we cannot expect a physiological explanation of the action of the brain in animal life, like that of the other viscera. In these the causes and effects are of the same kind.—When the heart causes the blood to circulate, it is one motion which produces another motion : when the stomach converts the food into chyle, it is the heat, moisture, gastric, and slow comprehension of its muscular coat, which unite to produce, at the same time, a solution and trituration, greater or less, according to the species of animal, and the nature of its food.

The functions of the brain are of a totally different order. They consist in receiving, by means of the nerves, and in transmitting immediately to the mind, the impression of the senses ; in preserving the traces of these impressions, and in reproducing them with greater or less promptitude, distinctness, and abundance, when the mind requires them for its operations, and when the laws of the association of ideas recall them ; lastly, in transmitting to the muscles, always by means of the nerves, the desires of the will.

Now these three function suppose the mutual, but always incomprehensible influence of the divisible matter and the indivisible mind (moi) ; a hiatus in the system of our idea never to be supplied; an eternal stumbling-block of all our philosophies. But there is another difficulty not necessarily connected with the former. We not only do not comprehend, and never shall be able to comprehend how any traces impressed on our brain can be perceived by the mind, and produce images in it ; but however delicate our researches, these traces are in no way visible to our eyes, and their nature is perfectly unknown to us ; although the effect of age and of disease upon the memory does not permit us to doubt of their existence, or of their seat.

It seemed, at least, that the action of the nervous system upon the organic life, would be more easily explained, as it is entirely physical, and we might expect by means of investigation, to discover in this system some texture, some intermixture, or direction of parts which would render it more or less analogous to the vascular or secreting organs. There was especially no reason to doubt, that it would be possible to unfold their different portions, to assign their connections, their relations, and respective terminations, as easily as in the other systems.

This, however, has not happened. The texture of the brain, of the spinal marrow, and of the nerves, is so fine, and so soft, that all that has been hitherto said of them is blended with conjectures and hypothesis ; and the different masses which compose the brain are so thick, and have so little consistence, that the greatest dexterity is required to show all the parts of their structure.

In short, none of those who have examined the brain, have succeeded in establishing a rational and positive relation between the structrue of that organ and its functions, even those which are most evidently physical : the discoveries hitherto made known with regard to its anatomy, are confined to some circumstances regarding the form, connections, or texture of its parts which had escaped the observation of preceding anatomists; and whenever any one has supposed that he had proceeded farther, he has only introduced, between the well known structure and its commom effects, some hypothesis, scarcely capable of satisfying for a moment even the least sceptical minds.

All the actual discoveries which have hitherto been made, may be reduced to new methods of dissecting the brain, new connections and directions perceived between its different masses, and the organic elements composing them, or new peculiarities observed in some of its parts.

We are far, however, from despising these results; they direct us to the only route which may at last conduct us farther; and although we do not yet know the whole length of this route; we are satisfied, that every step made in it brings us nearer to its termintion, by some fraction of its strength.

We proceed, then, to explain and examine, under these three heads, of method, connection, and peculiarities, the discoveries announced by Drs. Gall and Spurzheim.

Anatomists know that there are three principal methods of demonstrating the brain.

The one most generally practised in the schools, and described in books, is that of Vesalius, which consists in removing successive layers cf this organ, and in pointing out what presents at every cut. It is the one the most easily performed for the sake of demonstration, but the most difficult to be followed in the mind. The true relation of those parts, which we see always cut, escapes, not only the pupil, but the teacher slso; it is almost as if one were to divide the trunk of the body by successive sections to point out the position and form of the lungs, heart, stomach, &c. This method, however, is still almost the only one which is employed in the work of Vicq-d'Azyr, the most splendid, and one of the most vlauable which has appeared on the subject of the brain.

The second mode, which deranges much less the organ, the structure of which it is intended to explain, is that of

Willis, which, as far as we can judge by the obscure description of Galen, very much resembles that employed by the ancients. After removing the *pia mater*, the posterior lobes of the brain are raised, we penetrate between the *tubercula quadragemina* and the *fornix*, and cut its anterior pillar; after detaching the lateral parts of the hemispheres, they are turned forwards : in this manner we have a good view of the under part of the *fornix* and *corpus callosum* : we preserve entire, the great and small tubercles of the interior; but the thickness of the hemispheres renders it more difficult to practise this mode in man than in other animals.

The third method was very long ago sketched by Varolius, and afterwards more fully detailed by Vieussens. By this mode the under part of the brain is first examined, and the *medulla oblongata* is followed across the *pons varolii*, the *thalami optici*, and *corpora striata* ; its fibres are seen expanded to form the hemispheres ; it is even possible to stretch out the hemispheres by removing their lateral attachment to the crura cerebri ; to divide longitudinally the spinal marrow and the cerebellum ; and then each half of the former is seen forming a pedicle, which is implanted into the side of the hemisphere, as the stem of a mushroom is fixed into its *pileus*.

This mode possesses the very great advantage of allowing us to follow more easily the direction of the medullary fibres, the only circumstance which can afford us any idea with regard to the course of the cerebral functions ; and it is probable, that it would have been more generally adopted, if Varolius had not represented it in a very coarse figure, and if the work of Vieussens had not always remained, for what reason it is difficult to determine, in a kind of discredit which it did not deserve.

It is nearly this method of Varolius which is followed by Drs. Gall and Spurzheim, and which a part of their memoir is intended to defend ; a labour, indeed, very useless, for so complicated an organ as the brain, ought to be examined on every side ; it must be cut into every way ; and whenever any plan is discovered which points out some new circumstance, it furnishes an important addition to anatomy.

We shall therefore judge of their mode by the results; and, for that purpose, we shall detail and compare them with those which have been obtained by preceding anatomists.

It is well known, that the opinion most generally received with regard to the minute organization of the brain, is, that the cortical substance of the hemispheres, and of the cerebellum, being almost entirely vascular, is a sort of secreting organ: that the medullary substance, almost every where of a fibrous appearance, is a collection of excreting vessels, or at least of conducting filaments; that all the nerves are emanations from this substance, bundles of these vessels; that the *medulla oblongata*, and spinal marrow, is itself a bundle larger than the others, from which the different pairs of spinal nerves go off in succession: lastly, that the nerves called cerebral, are the first which go off from the great medullary mass of the encephalon; consequently, it is believed that all the influence of the nervous system upon organic life, as well as all the efforts of the will, descend from the brain along the nerves; and that the impressions received from the external senses, re-ascend by the same route; but by a singular contradiction, at the same time that they make the medullary substance, and consequently the nerves, originally extend to the whole of the cortical substance, several physiologists think themselves obliged to search for some circumscribed place from which all the nerves emanate, or, what signifies the same thing, in which all the nerves terminate; in short, for what anatomists call the *seat of the soul.*

It must be allowed, that this has been for a very long time the opinion most generally received, and that it is still very prevalent; although the most prudent philosophers have never brought it forward but as an hypothesis very slightly supported by facts.

Several of its partizans, however, have allowed themselves to be led into doubts and contradictions. Haller, for example, says in one place, that he cannot suppose that medullary fibres originate any where else but in the brain;* in another, that every nerve comes ultimately from the medulla of the brain or cerebellum;† while, in a third place,‡ he supposes that the cineritious matter of the spinal marrow may form them in the same manner as that of the brain.

In fact, this distribution of the cineritious matter in different parts of the system, was one strong argument against the exclusive importance ascribed to the encephalon, and to it might be added several others.

It might be remarked at all times, that the action of the nerves upon organic life continues for some time after the brain no longer contributes to it. Well known experiments upon insects and worms proved, that if in man, and the other animals where the brain is very large, this organ is necessary to the functions of animal life, it is not always so in those species where it is of less size, and that in some of these we may even instantaneously produce, by dividing it, two centres of volition and sensation.

It has also been known for a very long period, that the spinal marrow does not diminish in proportion to the nerves which go off from it, as it ought to do if it were only a bundle of these nerves sent from the brain; that, on the contrary, it swells at certain places, from whence the largest nerves go off. Prof. Soemmerring has recently remarked, that the size of the *medulla oblongata* in animals is not in proportion to that of the brain as it ought to be, if it were a congeries of excretory tubes from the brain, but that, on the contrary, it is often in an inverse proportion; the successive investigations of Monro, Prochaska, Reil, have given us ideas of the structure of the nerves quite different from those which we would be obliged to form if we were to derive all of them from the medullary substance of the encephalon, and by it from the cortical substance. Several physiologists, therefore, within these few years, have again begun to consider the nervous system as a net-work, all the portions of which contribute, in a certain degree, and especially according to their size, to the organization and functions of the whole ; and not as a tree which, having only one trunk, distributes itself into branches and twigs, after the manner, for example, of the arterial system.

---

* Phys. iv. p. 385. † Ibid. p. 393. ‡ Ibid. p. 384.

Drs. Gall and Spurzheim, in adopting this opinion, do not adduce new proofs of it, but only repeat those which we have stated, and which had been brought forward many years before.

It appears, that in Germany and other places, various objections have been made to their system which we would not have made, but which they have taken the trouble to answer.

When they, for example, described, that in the fœtus without a head, the nervous system performs its functions of organic life without the assistance of the brain, it was opposed to them, that the acephali are only fœtuses where the brain has been destroyed by the effects of dropsy. This objection, which is true in some acephali, certainly does not apply to all ; and it is by no means rare to see some which have been completely developed, although they do not show the smallest mark of having ever had either a head or any of the upper parts of the trunk.

We, therefore, readily agree with Drs. Gall and Spurzheim, with regard to the general idea which they entertain, as well as a great many anatomists, of the nervous system. But while they, with so many others, consider it as a net-work, they have some particular ideas concerning the links and knots of which this net-work is composed ; and here the part of their doctrine begins which is peculiar to them.

As far as we have been able to understand, it appears to us, that it may be reduced to the following heads, or propositions :

1. The cineritious matter is the *matrix of the medullary lamenfits ;* wherever it exists these filaments are produced ;

it exists wherever it is produced. Whenever a medullary bundle crosses cineritious matter, it is enlarged by filaments received from the latter, and none of these bundles enlarge without the concurrence of this matter, whether it form a sensible enlargement, or only follow and accompany the medullary bundle.

2. The spinal marrow is not a bundle of nerves descending from the brain. The spinal nerves rise by filaments, some of which ascend, others descend; this is observed especially in brute animals. The cineritious matter of the internal part of the spinal marrow is the matrix of these filaments; the spinal marrow swells whenever it gives off a pair of nerves, and the more the larger these nerves are. Thus the spinal marrow of the larger animals, as well as that of insects and worms with red blood, is only a series of ganglia, which give off nerves; but all these ganglia communicate with each other.

3. The nerves generally called cerebral, and which arise from the under part of the encephalon, and chiefly from the medulla oblongata, do not come from the brain any more than the others; on the contrary, when they follow separately the roots of each of these into the substance of the medulla oblongata, we see that they re-ascend from the medulla towards the point where they appear externally, and that they do not descend from the brain, to pass through the medulla oblongata.

4. The brain and cerebellum are themselves only developements of bundles which have come from the medulla oblongata, in the same manner as the nerves proceed from it.

The brain, in particular, arises principally from the bundles called pyramidal eminences, which cross each other in passing out of the medulla oblongata, each going to the opposite side to that from which it comes: they swell out first in crossing the *pons varolii*, again in passing over the tubercles called *thalami optici*, and a third time in those tubercles called *corpora striata*, always from the addition of medullary filaments, which the cineritious matter, contained in these three parts, adds to those they originally consisted of. The cerebellum comes from bundles called *processus cerebelli ad medullam*, or *corpora restiformia*, which receive an addition of filaments once only from the cineritious matter of what is called *corpus ciliare*.

5. These two pairs of bundles, after receiving these additions, after having assumed consequently a diverging direction, terminate each in two large expansions, covered externally by cineritious matter, which only here deserves the name of *cortical* substance; and these expansions folded in various ways, form what are called the hemispheres of the brain, the lobes, and the *vermiform prosesses* of the cerebellum.

6. From the whole extent of these expansions, other medullary filaments are produced, which converge from the two sides of the brain and cerebellum, towards the middle line, where the filaments of the one side unite with those of the other, and form what are called the *commissures*.

The corpus callosum, the fornix and its appendages, form the largest of the commissures of the brain; the anterior commissure is that which joins the middle lobes. The commissure of the cerebellum is compossed of the transverse layers of the pons varolii.

7. When we remove or lacerate the converging fibres which go to the corpus collosum, and which serve as a ceiling to the lateral ventricles, there only remains, under the cineritious substance, a medullary part lining it, and following all its folds; and far from forming a solid mass, as has been hitherto supposed, there is always in the middle of each evolution of the brain and cerebellum a solution of continuity, and by employing proper care, we may unfold this portion of the medulla, as we would unfold the cineritious substance if it were alone. In a word, each convolution is a kind of small purse or canal, closed externally by a double layer of cineritious and medullary matter, and on the side of the ventrical by the converging medullary fibres.

8. As the pairs of bundles forming the brain and cere-

bellum have their commissures, those which form the nerves have often theirs also, which may be very easily demonstrated in the second, the fourth, fifth, and seventh pairs, and very probably in the others.

9. The ganglia spread over the whole body are small masses of cineritious matter, which certain nerves, pass through and where they are strengthened as the pedunculi of the brain are increased in the thalami optici and corpora striata.

These two pairs of tubercles then are true ganglia to these pedunculi. The cineritious matter of the cortical part of the brain and cerebellum may likewise be regarded as a ganglion of the commissures, or converging fibres. That of the inner part of the spinal marrow forms, in like manner, the primary ganglia of the spinal nerves. The cerebral nerves have probably each of them a particular ganglion, and it is easy to demonstrate it in several of them. The cineritious matter, and consequently the ganglia, may therefore be compared to the mucos expansion which covers all the extremities of the nerves of the skin, of the intestines, and even the pulp of the labyrinth, and the sort of mucous varnish which covers the retina.

10. From these nine articles, all merely anatomical, all more or less susceptible of being verified, a tenth results, which completes, and is the essential character of the anatomical doctrine of Drs. Gall and Spurzheim, viz. that each pair of nerves forms a particular system; that all these systems communicate together, and re-unite in the great cord of the medulla oblongata and spinal marrow; and lastly, that the brain and cerebellum, far from being the origin or source of this cord, are, on the contrary, an appendix, or sort of *diverticulum*, reserved for certain functions, but receiving an influence from all the parts of the cord, and exercising one upon them by means of their communications.

We do not think that any anatomist can perceive any obscurity in this new explanation of the ten principal articles premised by the authors of the memoir which we are examining; they have, besides, acknowledged them to be the true expressions of their opinion

It only remains for us, therefore, to say, how far they appear to us to be true and new; this we shall do separately with respect to each of them, and with the more interest, as their will result from our examination a kind of treatise, in which the structure of the brain will be considered under different views, more or less important, and abounding with extensive consequences.

But, before proceeding to this, justice requires us to repeat the declartion of Drs. Gall and Spurzheim, that they do not pretend to have discovered many new facts; but that the chief merit which they claim consists in the connection which they have been the first to establish between the known facts, and in the general conclusions which they have deduced from them.

The first article which ascribes as a function to the cineritious substance the giving rise to the medullary filaments, or, as the authors of the memoir express it, the being the *matrix of the nerves*, is, indeed, only another expression for the opinion generally received. Disputes have existed with regard to the texture of that substance. Malpighi supposed it to be formed of small follicles; Ruisch, perhaps with more justice, only admitted a vascular net-work; others are of opinion, that there is also, besides the vessels, a particular substance; but it has been almost always agreed to regard it as a secreting organ, and the fibres of the medullary substance as the excreting organs of the substance which it secretes; it was therefore necessary that these fibres should arise from the cineritious matter. The physiologists who do not believe the nerves to be hollow, but suppose that they have the power of conveying a fluid in the manner that the metals conduct electricity, do not all deny, on that account, that the nerves receive their fluid in the cineritious substance; hence they also think that they arise from it. Those, lastly, who ascribe to all the portions of the medullary matter a secreting power, do not pretend to deny, what the eye demonstrates, the close adhesion of the medullary to the cineritious matter of the cortical part of the hemispheres, and the immense quantity of filaments which go out as so many radicles, the cineritious portions of the corpora striata and thalami optici, &c.

Our authors have, therefore, nothing particular in the function which they ascribe to the cineritious substance. Even in generalising this function to all the parts of this substance, they only express, in more positive terms, what, as we have seen above, Haller suspected to be the case with regard to the cineritious portion of the spinal marrow.

Since this opinion is admitted by so many anatomists, there must be powerful reasons for it; in fact, besides what the eye informs us of the intimate connection of the two substances, the number of arteries which go to the cineritious substance, and which seem almost entirely to compose it, can have scarcely any other object than a copious secretion.

The quantity of cineritious matter dispersed over all the parts of the nervous system, and to which the authors of the memoir have the merit of recalling our attention, would, perhaps, explain sufficiently the functions which the parts of the system exercise without the concurrence of the brain, and would dispense with the necessity of having recourse to a particular power of secretion in the medullary substance, or even in the sheath of the nerve, as Reil supposes.

The *second article* draws a comparison between the spinal marrow of the superior animals, and that of insects and articulated worms, or those with red blood.

It is well known that, in the two latter classes, the brain is scarcely larger than the ganglia, or knots of the spinal marrow from each of which the pairs of nerves go off; that it is from the size of these ganglia, and from their separation, as well as from the smallness of the brain, that we endeavour to explain the divisibility of the individual, which is remarked in all these species, at least for some minutes, and which goes so far in some, such as the *earth-worms* and the *naides*, as to make two lasting individuals from one, by means of division.

Nothing resembling this had been perceived in man, whose spinal marrow had no sensible contraction, and only swells out at those places from which it gives off nerves to the arms and thighs; but Drs. Gall and Spurzheim have showed us a preparation of the spinal marrow of a calf, in which is observed a kind of slight swelling between each pair of nerves. It would be curious to ascertain exactly in what animals this structure is found, and if it has any relation with the power of executing certain voluntary actions, without the brain; if the turtles, for example, which live and walk several months successively without this viscus, have the spinal marrow more knotty than other animals with red blood, &c.

One of our number has begun inquiries with this intention, which have not afforded him sufficient results to be laid before this class; but he has already ascertained that there are no sensible knots in quadrupeds even very nearly allied to the calf.

The *third article* is subdivided, for examination, into as many heads as there are pairs of nerves.

The general result which the authors propose to prove is, that all the nerves arise from the medulla oblongata or spinal marrow, and not from the brain.

There is no difficulty as to the spinal nerves, which are only conjectured to come from the brain, but the roots of which undoubtedly no human eye can trace into it, nor even perceive a tendency to run into it.

There can be no more with regard to the last pairs of the encephalon, counting from the par vagum, and under it, for they arise by transverse filaments, as the spinal nerves, although they have not two bundles; and no anatomist has observed these filaments reflected towards the brain after having entered the spinal marrow.

There is still less doubt with regard to the nervus accessorius of Willis, which evidently re-ascends.

We have only, therefore, to attend to the first eight pairs, reckoning the facial nerve as a separate pair.

The *seventh* pair of Willis is, in fact, now generally acknowledged as forming two nerves, distinct at their origin as well as in their course. The soft portion, or the *auditory nerve*, arises transversely from the *corpus restiforme*, called also *processus cerebri ad medullam.* This nerve has been for a long time considered to be formed by the small white filaments traced over the ceiling of the fourth ventricle, and this is still the opinion of Haller,* of Vicq-d'Azyr,† and of Soemmerring.‡ However, as these filaments vary in number, and even in direction—as a part of them are sometimes seen reascending towards the corpus restiforme, or passing through it towards the pons varolii,§ and as it is not very rare to find them wanting, their continuation in the auditory nerve has begun to be doubted. Prochaska,|| the two Wenzels,¶ &c. have formally denied it. The latter** and Dr. Gall have farther remarked, that these Striæ are generally wanting in animals.

The two Wenzels†† observed, in 1791, for the first time a small grey band slightly prominent, placed also transversely upon the corpus restiforme, and which constantly covers a part of the base of the auditory nerve, which it unites with the fourth ventricle. Prochaska is hitherto the only one who has given a representation of it.‡‡ It is likewise observed in animals; and Dr. Gall, who adopts, in this respect, the opinion of the Wenzels, remarks that it is the more swelled in every species the larger the ears are, and the more acute the hearing is.

In the *horse*, the *stag*, the *sheep*, it is a tubercle almost as large as the eminence called *testis*.

We have found this circumstance to be true.

---

* Phys. t. iv. p. 225.

† Explication des Planches, p. 95.

‡ De Frabrica Corp. Hum. t. iv. p. 256.

§ We have had a very marked example of the latter structure in the course of the researches occasioned by this memoir.

|| Oper. Min. t. i. p. 338.

¶ Prod. p. 22.

** Prod.

†† Ibid.

‡‡ Oper. Min. t. i. tab. iii. fig. 1.

It is besides evident, that if the origin formerly admitted were the true one, the auditory nerve would not, therefore, arise less transversely from the medulla oblongata, and that its visible roots would always come rather from below upwards than from above downwards.

The *facial nerve*, or portio dura of the seventh pair, and the *abductor* or the sixth pair, are then the first which can leave any doubt, whether they arise from the medulla oblongata, or from the brain, from behind or from before.

In man they both arise from the body of the medulla oblongata, immediately behind the posterior edge of the pons varolii, and so near to it, that several anatomists make them derive a part of their filaments from it.

The *facial* nerve, in particular, goes off some lines farther outwards than the other, at the angle formed by the pons varolii and corpus restiforme, at about a line from the point where the *auditory* nerve is detached from the corpus restiforme, which it had, as it were, surrounded.

The *abductor* seems to arise from the fissure which separates the pons varolii from the pyramidal eminences; and there are some anatomists who derive all its roots from the pons, others from the pyramids, others from both parts. There are others again who are not explicit with regard to this point.

According to the idea generally adopted, that the nerves descend from the brain, M. Soemmerring supposes* that the abductor nerve has its roots in the pedunculi, and that they are separate from them, turning backwards, after the pedunculi have crossed the pons varolii to form the pyramidal eminences. This is nearly the opinion of Vieussens,† but it is evidently the result of hypothetical reasonings, and not of actual observation.

To ascertain the true direction of the roots of these nerves, it is necessary to have recourse to the herbiferous animals, in which the pons varolii does not cover them, since it is not so large as in man.

---

* De Bas. Enceph. p. 140.

† Neurogr. Univ. p. 176.

This is what Drs. Gall and Spurzheim have done, and they found at once that the *abductor*, in them, arises some way behind the pons varolii, and appears to be the continuation of a small bundle which ascends between the corpus pyramidale and olivare. The filaments which produce it are longer at the back part and shorter before, so that they have, in miniature, the same arrangement as those of the accessorius of Willis. There is no reason, then, to suppose that it descends from the brain.

This observation concludes the discussion whether or not the nerve receives some filaments from the pons varolii; since it is only on account of the breadth of the pons in man that it approaches to its posterior edge.

We have not found any positive traces of this remark in the authors whom we have consulted, but we have convinced ourselves that it is true with respect to the herbiferous animals; and one of our number had even examined it some time ago in the horse. In carnivorous animals and the ape, the pons varolii and the sixth pair have a greater resemblance to what is observed in man.

With regard to the *facial nerve*, a transverse medullary band is seen in the herbiferous animals, behind the pons varolii, which begins exactly at the outer edge of the abductor, and passes over the root of the *par trigeminum*, or where it is continuous with the auditory nerve. The facial nerve appears to pierce the band obliquely from behind forwards. Thus it would seem to arise from the under part of the medulla oblongata, almost in the same manner as the auditory nerve arises from the upper part, and two pairs of nerves would be formed, the origin of which is, in fact, separated from each other by the whole thickness of the medulla oblongata, although they afterwards approach so as to touch each other.

Nor have we remarked that any author has pointed out this fact before Dr. Gall; but we are certain of its accuracy, and one of us had seen it, and delineated it some time ago, in the stag, horse, sheep, and rabbit.

The brute animals also show more clearly than the man the origin of the *par trigeminum*, or fifth pair.

It is generally said simply to arise from the lateral parts of the pons varolii, or from the extremity of the peduncali cerebelli. This is still adhered to by Vicq-d'Azyr* and Meckel.† Haller reckons this pair in the number of nerves, which may come both from the cerebrum and cerebellum.‡ It is, however, ascertained, that it neither comes from the one nor the other, and it may be traced deep into the medulla oblongata, to nearly an inch farther back than where it comes out.

Santorini asserts§ that he had traced the roots of it to above the corpora olivaria, and says, that it is more astonishing to see this nerve ascend from below, than the *accessorius of Willis ;* but he then supposes that a part of the fibres of the pedunculi which do not enter the corpora pyramidalia, they being, in fact, too small to contain them all, go on further, and then those, among others, are reflected which form this nerve ; a very gratuitous supposition, and which nothing sensible to the eye can justify.

M. Soemmerring seems not to have understood Santorini at the time he wrote this treatise *De basi encephali ;*‖ but he relates, that ¶ accident had made him afterwards trace the origin of this nerve into the substance of the medalla oblongata, even as far as the bottom of the fourth ventricle ; and, according to his favorite hypothesis, with regard to the seat of the soul, he conceives that the primary roots of it are bathed in the water of the ventricle.

Dr. Gall follows, in a steady and sure manner, this deep and low origin of the par trigeminum, as far as between the *corpora olivaria* and *restiformia.* He shows besides, that the breadth and thickness of the pons varolii in man, where the obstacles to its being discovered sooner. In fact, in the herbiferous animals, in which the pons is much narrower, the roots of the trigemini are easily traced, under a part of the pons, and under the transverse band placed behind, which we have seen to be partly the origin of the facial nerve as far as the longitudinal bundle which runs along the outside of the corpora olivaria.

---

* Explication des planches, p. 52. † No. 46. et. 47.
‡ Phys. t. iv. p. 387. § Observat. Anatom. p. 64, 65.
‖ P. 135. ¶ De fabric. corp. hum. p. 212. No. 7.

We have confirmed these two observations; and in repeating the second in several animals, we have ascertained that it is found neither in the ape, nor in several of the carnivorous animals in which the nerves go off as in man; but always because the pons varolii in them also is broad. With regard to the first, it has appeared to us to be so certain, that we cannot help saying, that Vicq-d'Azyr has been deceived in deriving the roots of the fifth pair from the pedunculi cerebelli.* In examining always with the knife, he must have divided them, and lost sight of them too soon.

Every one knows that the *nervus patheticus*, or the fourth pair arises transversely from the valvula Vieussenii, behind the *testes*. There is nothing which could make it be derived from the great medullary mass of the brain.

The *motor oculi*, or third pair, comes from the pedunculus cerebri, towards its internal edge, where it touches the *perforated cineritious space*, lying between the two pedunculi and the two processus mamillares, and receives some filaments from it. In man its roots are arranged in a line, nearly in the direction of the pedunculi, the posterior roots are the longest, judging even by the external appearance; they proceed then rather from the posterior than the anterior part; but if a slight cut be made into the peduncle, this fact becomes still clearer. The greater part of these roots may be followed as far as under the pons varolii. A part of them is lost, or rather is formed around the black coloured part of the pedunculi. This argument is very well represented by Vicq-d'Azyr, *pl.* xxxi. *fig.* 2.

In other animals the roots are more transverse, and more perpendicular; such, at least, we have observed them to be in the horse and the sheep.

---

* Mem. de l'Acad. des Sciences, 1781. p. 565.

The *optic nerve* is very generally considered as proceeding from the thalami optici ; because its roots are spread out upon them in the manner of a thin membranous expansion, which almost entirely covers them. However, anatomists have not been wanting, who supposed that they could trace at least a great part of these, as far as the *nates ;* such as Mongagni, Winslow, Zinn. Santorini* describes this origin very carefully, and adds another, which he makes to come from the testes ; his pupil and editor, Girardi, confirms it,† Vicq-d'Azyr, who was also very well acquainted with these connections of the optic nerves with the tubercula quadrigemina, pretends, however, that they have likewise other roots in the substance of the thalami, which in the form of innumerable filaments, join the nerve in a great part of the course which it takes in surrounding the crus cerebri ; and he boasted of this discovery.‡ But this appears to us to be a deception, into which he may have been led by his mode of making parallel incisions. It is very true, that a number of white filaments arise from the cineritious substance of the thalami ; but they do not appear to us to go to the optic nerve. On the contrary, they are added to the bundle which comes from the pyramidal eminences, as we shall mention presently. Drs. Gall and Spurzheim have found, out a mode of dissection which demonstrates this very well, and which we shall mention again.

They suppose then that it is possible, at least in several animals, to remove from above the thalami, without affecting them, the medullary expansion of the roots of the optic nerve, and to follow them as far as the internal part of the nates, where they form a white lamina which occupies the centre of these tubercles.

The last point is certain ; with respect to the first, as it can only be performed with the assistance of the handle of the scalpel, it is liable to the same doubt as all similar operations which may be tried on the brain.

---

* Observat. Anatom. p. 63. † Septemdec. tab. p. 34. Acad. de. Sc. 1783. p. 529.

Our anatomists farther remark, that in those individuals in which the optic nerve of one side is weakened and thinner, the corresponding tubercle is also smaller; and that in those species which have large optic nerves, the nates are of a larger size; but that frequetly in these cases the thalami are smaller.

The medullary tract which comes from the nates, meets in its course the tubercle called *corpus geniculatum externum.*

That which comes from the *testes* divides the first at the *corpus geniculatum internum*, and seems to slip under it, to cross it in passing forwards: thus Drs. Gall and Spurzheim do not believe that it belongs to the optic nerve; they have even for a long time thought that it forms the external root of the *olfactory*, which is indeed very nearly in that direction; but they have never been able to trace its continuity.

One of our members has made several researches on this part of the brain, which appears to have been only well known to Santorini and Vicq-d'Azyr, but which has never been properly delineated.

It is in fact, that in all quadrupeds, the principal bundle of the optic nerve goes from the nates to the *corpus geniculatum externum.*

It is also certain, that another bundle comes from the testes, which makes an angle with the first; and which, after swelling out to form the *corpus geniculatum internum*, seems to pass under the first bundle, and proceed further, but is soon lost to the eye and the knife.

Lastly, It is ascertained, that both the *testis* and the *corpus geniculatum internum*, are much larger in the carnivorous than in other animals; and this fact would be favourable to the idea, that they contribute to form the olfactory nerve, which is so conspicuous in that class.

But we think that we have seen in the ape, that the *corpus geniculatum internum* receives a bundle from the *nates*, as well as from the *testes*, and forms by their re-union a root of the nerve, which joins only very low down that which comes as usual from the nates above the *thalamus opticus.* Drs. Gall and Spurzheim have themselves besides remarked, what one of us pointed out long ago, that the dolphins and porpoises, which have no olfactory nerve, have, however, very considerable *testes.*

These ainmals, likewise have the corpora striata as the others, which shows that these bodies do not possess the function attributed to them, of forming the olfactory nerve.

The first pair, then, will be the only one, the roots of which cannot yet be traced to the medulla oblongata, and which does not agree as yet clearly with the rule established in the memoir which we are examining.

Dr. Gall explains, conformably to the law mentioned in his first article, the enlargement of the optic nerves below their conjunction, by numerous filaments sent to them by the cineritious lamina interposed at the fore part of that conjunction; which filaments have been accurately described, and carefully delineated, by Vicq-d'Azyr.*

A strong objection was made to the origin, which our anatomists assign to the optic nerve, drawn from the structure of birds, which, it was said, have no *nates*, although their eye and optic nerves are of a very large size; but their answer is convincing. What Willis, Collins Haller, and other anatomists have called thalami optici in birds, are in fact the *nates.* The true thalami optici are situated before, with their third ventricle, the pedicles of the pineal gland, and the two commissures in their usual place; in a word, they are in every respect similar to those of the quadrupeds, in proportion to their relative size: the supposed thalami of Haller are, on the contrary, between the posterior and the commissure and the valvula Vieussenii; the aqueduct of Sylvius passing between them; it is with it that the ventricles communicate, which are peculiar to them in this class.

We have proved the truth of this important remark; it can admit of no reply. It is the more the duty of the reporter to acknowledge it, as he had adopted the common error in his works.

---

* Mem. de l'Acad, 1783. p. 548. et. pl.xiii. fig. 1 et 2; and in his large work, pl. xxi. in all the figures.

Now, as the tubercles in question evidently give rise to to the optic nerves in birds, they confirm the origin which they ascribe to these nerves in the mammalia, and in the man, instead of invalidating it.

We may here repeat the elegant remark made by Vicq-d'Azyr, that these tubercles have a ventricle in birds in which the sense of sight is the most acute ; as the olfactory nerves in the mammalia, in which the sense of smell is superior to the others.

We proceed to ARTITLE IV. in which our authors explain the relation between the medulla oblongata, and the brain, and the cerebellum.

The continuity of the medullary fibres of the pyramids across the pons varolii, with the crura cerebri, and these across the *thalami optici* and *corpora striata*, into the medullary substance of the hemispheres, was well known to Vieussens, who had also given the very just appellation of posterior corpora striata to the thalami optici ; but the figures,* in which he represents this important point of the anatomy of the brain, are very coarse; they only show simple filaments, which went on enlarging and seperating from each other, which is far from being the case.

This interesting point of view was afterwards almost entirely overlooked, because they contented themselves with cuts made in the superior part of the brain. Monro† and Vicq-d'Azyr‡ again introduced it; the latter especially represented this continuity in two very beautiful plates, although, perhaps still a little less exact than is necessary, because the preparer had not taken care to bend in his incision according to the direction of the filaments.

To these horizontal incisions already made by the three authors we have just quoted, Drs. Gall and Spurzheim add

---

[*] Neurogr. Univ. p. 88 et. 89.

[‡] Nervous System, t. vii. fig. 1.

[†] Grand ouvrage sur le cerveau, pl. xxii. et xxiii.

a vertical one, which has the merit of explaining, according to their view, how these medullary bundles enlarge, and of pointing out the true termination of the filaments of the thalamus opticus, which Vicq-d'Azyr thought he had traced into the optic nerve.

This cut passes through the middle of the pyramidal eminence, of the crus cerebri, of the thalamus and corpus striatum of one side, obliquely forwards and outwards.

The bundles of the pyramids are there distinctly seen intermixing with those of the pons varolii, and with the cineritious substance which mixes there with them, and supplies some additions; passing from thence into the crus cerebri, they again receive filaments from the *processus cerebelli ad testes*. At one place, under the thalamus opticus, they unite into a white mass, to which innumerable filaments of the internal part of the thalamus are joined at acute angles, pointing forwards. It is essential to remark the latter circumstance; it proves that the thalami send their filaments forwards, and not backwards, as Vieussens had supposed; it points out also, that it is not into the optic nerve that these filaments go, as Vicq-d'Azyr thought.

The white mass then becomes stronger, and divides into a great number of diverging pillars, which constitute the white streaks of the middle of the corpora striata: the cineritious matter of the superior surface of these bodies gives also a great number of small filaments, as the thalami had given; at last, all these fibres are dispersed in the medullary mass of the hemispheres, where we shall follow them presently.

The two transverse whitish arches which are seen in the horizontal cut, and part of which Vicq-d'Azyr has expressed in his plate, are the places to which the greater number of filaments come from the superior parts of the thalami and corpora striata.

Such is the faithful description of what is perceived by the eye; one of us has delineated all this arrangement in man, quadrupeds, and birds, in which the essential points are nearly the same.

We are aware that there is no better reason for saying that the great fibrous bundles go from the pyramids to the hemispheres, than from the hemispheres to the pyramids; since the nervous influence proceeds in both directions.

But one may and ought to inquire which direction the small fibres of the thalami and corpora striata take. Are they supplied by these tubercles to enlarge the great medullary bundle, or are they given off from the medullary bundle to sink into these tubercles? This latter opinion certainly could have no probability, and no one will condemn Drs. Gall and Spurzheim for adopting the opposite opinion.

They would then be in the right in this sense, when they say the medullary bundles go on always enlarging from the pyramids to the hemispheres.

But whence do the inferior extremities of the bundles, these same pyramidal eminences, come; or, whither do they go?

They cross each other about two fingers breadth behind the pons varolii, and immediately disappear behind that point, losing themselves on both sides in the two cords composing the inferior surface of the spinal marrow.

This is one of the most interesting facts in physiology and pathology.

Every body knows how frequent it is to see a paralysis of one side, occasioned by an injury of the opposite side of the brain; physicians of every age have endeavoured to explain this fact by a decussation which they vaguely supposed in the fibres of the brain, or in the deepest roots of the nerves.*

However, we see, almost every where, only transverse fibres, commissures, and not crossed fibres.

---

* Arétée de Caus. et Sig. Morb. lib. i. cap. 7. p. 34. B. edit. Lugd. Bat. 1731. Nervi ab initio enati protinus ad oppositos transeunt, se invicem permutantes in figuram literæ X.

There is only one place, at the posterior extremity of the medulla oblongata, which presents a true decussation, which was discovered by Domenico Mistichelli, and very well described by him in 1709.† François Pourfour du Petit‡ likewise described it the following year, and was the first who made it known in France.

How has it happened that a point of structure so evident, adopted by Winslow,§ Lieutaud,‖ Portal, distinctly described,¶ and plainly delineated,** by Santorini, should have been doubted by the great Haller,†† recently denied by very skilful men, and confounded by others, among whom may be reckoned Vicq-d'Azyr himself, with that of the transverse fibres which re-unite, in their whole length, the lateral parts of the medulla oblongata?

It is probably owing to the want of a sufficiently clear description, and perhaps, also, because the place of the decussation must be often cut in separating the head from the body.

It will be impossible to be mistaken in it, after the demonstrations of Drs. Gall and Spurzheim. When we separate from each other the two inferior cords of the medulla oblongata and spinal marrow, we see that they are separated by a pretty deep fissure, the bottom of which is occupied by transverse medullary filaments. This fissure is only interrupted at one place, which is what now engages our attention, and which is only two or three lines in length. The fibres of the pyramidal eminence of one side form there three or four filaments, which decussate upon the fissure with the opposite filaments, as the hairs of a mat, and which are blended afterwards with the rest of the medullary cord, into which they thus enter obliquely.

---

† Trattatte dell Apoplessia, Roma, 1709, in 4to.

‡ Lettre d'un Medicin des Hopitaux du Roi, p. 12. Namur, 1710, 4to.

§ Traité de la Téte, No. 110.

‖ Anatomie Historique et Pratique, t. i. p. 591.

¶ Santor. Observ. Anatom. p. 61. xii.

** Ibid. § xvii. tab. II.

†† Phys. t. iv. p.

This decussation strikes the eye when we separate gently the edges of the longitudinal fissure of the medulla oblongata; because it is the only place where we cannot perceive the bottom of the fissure.

There is certainly some merit in having renewed the general knowledge of an important point of doctrine, which the doubt or denial of able men had caused to fall into oblivion.

Dr. Gall having established, as it appears, from this progression of medullary bundles of the brain across the pons varolii, the thalami, and corpora striata, his law of the increase of the medullary fibres by the cineritious substance, wishes to apply it to the cerebellum.

He has recourse here to the cineritious body of so singular a figure, which is found in the substance of the crus cerebelli, which has been called *corpus ciliare*, or *corpus fimbriatum;* the bundle called *processus cerebelli ad medullàm* would give rise to the cerebellum, after having been reinforced by the corpus fimbriatum, as the pedunculi cerebri are by the thalami optici, and the cineritious part of the corpora striata. But perhaps this analogy is not complete. The corpus fimbriatum is enveloped, and, as it were, immersed in medullary matter, instead of giving a passage to it; and we do not observe that it supplies it with any filament.

Some person will perhaps add, after the example of Vicq-d'Azyr,* that the brute animals have no corpus fimbriatum; but the truth is, that it is only of a smaller size; and as their cerebellum is also much less, the fact would be more in favour than against the idea of our anatomists.

Articles fifth, sixth, and seventh should be examined together. These three form the most particular points of the doctrine of Drs. Gall and Spurzheim; the seventh especially, relating to the possibility of unfolding the brain like a membrane, has made the most noise in the world; but, as is too commonly the case, almost none of those who have spoken of it have properly understood our authors; and those who have thought that they had found the fact in more ancient authors, had still less understood the circumstance itself, and the passages where they thought they saw it expressed.

---

* Acad. des Sciences, 1783, p. 471.

The terms in which we have expressed the ideas of Drs. Gall and Spurzheim, will make you already perceive that the question is not the unfolding the whole brain; they have expressly stated, at the meetings which we have had with them, that the parietes of the ventricles are such as they appear to be, and that they do not conceal any folds, except behind, towards the plaited margin, where their folds were, a long time ago, known to, and delineated by, Vicq-d'Azyr; only, our anatomists say these thick sides, formed by the converging fibres, are the only bands which retain the folds of the external substance; this is attached there as the folds of a plaiting, for example, are fixed to the stuff of a gown; if you remove the principal stuff, the folds will extend and form likewise a plain piece of stuff.

Your committee have examined, with all possible attention, the hemispheres of the brain, in order to judge of the truth of so new a doctrine.

They think that they can, in fact, distinguish two orders of fibres in the medullary matter, but they find it necessary still to confine very much the idea which might be formed of the unfolding, according to the expression which we have just stated.

We shall now explain, in succession, these two propositions.

When we trace with the knife the fibres which come from the crura cerebri across the thalami optici and corpora striata, we see that they intersect, at right angles more or less acute, those which go towards the middle line, and which form the corpus callosum and the fornix; it is even very easy to demonstrate their decussation in the inferior horn of the lateral ventricles; and the diverging fibres which come from the corpora striata, appear very evidently to form a layer external to the converging fibres which compose the corpus callosum.

But, does this external layer follow all the duplicatures of the still more external layer of cineritious matter, which is called cortical,—and does it unfold itself as the latter might be unfolded, if it were alone, and freed from all the white matter which fills it?

The one of these questions is evidently entirely independent of the other, and to be decided entirely by the testimony of the senses.

Taking, in the first place, the circumstance in the strict acceptation in which it seemed to be announced, we have used every effort to enable us to adopt or reject it with certainty; it would not be easy for us to make those who have not tried it understand what very great difficulties we have had to encounter. The medullary matter which fills the circumvolutions of the brain is so soft, that it sinks down by its own weight; however lightly one supports with the finger the convexity or the dorsum of one of these circumvolutions, its two sides separate horizontally, and each of them carry away a part of the medullary matter which filled up the interspace. The vessels are not ruptured, because they are for the most part placed in the direction in which the rupture takes place; and, besides, because they pass through this medullary matter on account of its softness, as threads would pass through jelly or pomatum. It appeared to us, then, impossible to prove that there was an actual solution of continuity; on the contrary, both with the naked eye and the magnifying glass, the two laminæ of the white matter appeared covered with small prominent points, with small filaments, which had quite the appearance of as many lacerations. We even tried to make the laceration begin so as to leave a thicker lamina of white matter on one side than the other; and it appeared to us that the separation was made as easily as in the middle.

The argument which the authors of the memoir draw from the example of *hydrocephalous patients* did not appear to us much more conclusive. An accumulation of fluid in the ventricles of the brain may slowly distend the parietes, efface the projection of the circumvolutions, and render thin the medullary matter which surrounds them, without the latter being unnecessarily unfolded; the dropsy of the kidney distends and lessens the substance of that organ to such a degree, as to make it resemble a membrane, without any one being tempted to suppose that it was unfolded. The phenomena of hydrocephalous patients, who, having preserved their intellectual faculties for a long time, proves nothing further; for, as we do not know with what part of the brain, nor with what circumstance of its organization these faculties are connected, we can draw no conclusion from it relative to the essential structure of the brain.

Farther, we have also examined hydrocephalous patients; the parietes of the ventricles, though extended, had the same appearance as usual, and the circumvolutions, although thinned and partly effaced, nevertheless preserved their internal solidity.

Such were the ideas excited in us by the first memoir of Drs. Gall and Spurzheim, compared with the objects themselves; but these anatomists have since transmitted to us an additional note, in which they point out new modes of ascertaining the facts, and in which they explain their mode of viewing them with more precision.

M. Spurzheim has repeated in our presence these new experiments; vertical slices of the circumvolutions, macerated in nitric acid, diluted with rectified alcohol, became hard, and were divided more easily at the middle line; the same took place when they were boiled for twelve or firteen minutes in oil; when one blows on such a slice, or directs a small stream of water on it with a syringe, the separation may be made very easily in the middle, and scarcely at all at the sides. In the latter case, especially, the two surfaces which are separated, remain smooth, and the vessels passing through them untouched, without showing any traces of fibres which might have gone from the one side to the other.

These facts are accurate, but perhaps they only prove that there is less cohesion in the middle of a circumvolution than in the rest of its capacity, and not that it is formed of two laminæ simply applied, and not adhering together.

In other words, it may be admitted, in our opinion, that the white or intermediate portion of each circumvolution, is formed of two parts, which adhere to each other more slightly than the particles of each among themselves; or the union of them may be compared, for example, to that of the two laminæ of the dura mater, but not as was supposed, to that of the two sides of an intestine compressed, except, however, that the medium of union is not cellular matter, as in the dura mater; but the medullary substance itself a little softened.

Besides, as this is a point of fact, admitting an appeal to the senses, we do not pretend to give to our opinion more authority than it ought to have; this question cannot be long of being examined by every anatomist, and will find as many judges as observers; it cannot fail then of being soon finally fixed.

It is a great deal more difficult to demonstrate these orders of fibres in the cerebellum, than in the brain; and it is by analogy, rather than by actual knowledge, that Drs. Gall and Spurzheim admit their existence there.

With regard to what they say of the commissures of the brain, and cerebellum, their ideas are not new, or different from what has been already advanced by a very great number of anatomists; we may also add, that there is nothing in them which is not very probable.

We find the same probability in the commissures which *article eighth* attributes to each pair of nerves. It amounts almost to certainty in all the spinal nerves, whose commissures are the transverse filaments of the spinal marrow. We may suppose, that the small band which unites the two facial and the two auditory nerves in animals, is concealed in man by the pons varolii; the two pathetics touch each other on the valvula Vieussenii; the two optic nerves, as every one knows, appear almost to be intermingled together anteriorly to the pituitary gland; besides, their roots must unite at the same time as the nates and testes on the aqueduct of Sylvius. There would then only remain the abductors, motores oculorum, and olfactory nerves, which would not have visible commissures. The anterior commissure of the brain also, is evidently united to the olfactory nerves in brute animals.

This generality of the commissures seems to assist in explaining the unity of action of the double organs.

The *ninth article* is one of those which have been the most contested by the anatomists of Germany, and which is in fact the most susceptible of being so. It establishes, in the first place, the generality of tubercles of cineritious matter for each pair of nerves; secondly, the analogy of these tubercles with those called ganglions; lastly, the analogy of these two kind of organs, both with the cortical matter of the brain, and with the mucous expansions of the organs of the senses.

That each pair of nerves belongs originally to some tubercle, or, at least, to some portion of cineritious matter of some form, may be very readily maintained, with regard to the spinal nerves, and as far up as the par vagum, since cineritious matter extends the whole length of the spinal marrow, although it is not possible to follow the roots of the nerves so far; it is even certain, with respect to the auditory nerve which arises from the small cineritious band in man, or from the much more marked tubercle which supplies its place in the greater number of brute animals; and with regard to the optic nerve, which has at least two of these tubercles, the nates, and the *corpus geniculatum externum*, and perhaps also two others, the testis and *corpus geniculatum internum*; the olfactory has at least one at the place where it rests upon the cribriform lamella of the ethmoid bone; but the eye can perceive nothing of a similar appearance in the other cerebral nerves in man, in the mammalia, and in birds, although the par trigeminum has a tubercle in fishes.

The probability of the analogy of the spinal ganglia, and of those dispersed in the nervous system of organic life, with the portions of cineritious matter ascribed to the primitive origins of each pair of nerves, is liable to difficulties of a very different nature.

Anatomists, among whom it is sufficient to mention Winslow, have undoubtedly, for a very long time, regarded the ganglia as little brains, as sources of nervous action, independent of the great brain; others, such as Willis and Vieussens, have at least considered them as reservoirs of the animal spirits; or, like Lancisi, as organs which may be compared to a heart, and adapted to communicate a more rapid motion to these spirits.

Scarpa, of late, as well as Mekel and Zinn, in former times, was only willing to consider them as subdivisions, reunions, and re-compositions of nerves, enveloped and strengthened by cellular substance, bathed in a reddish fluid, and sometimes intermixed with fat.

The existence of this cellular membrane, and the fat which is sometimes deposited in them, have been acknowledged by the most distinguished anatomists of the present times. They are very distinctive characters, which do not permit the substance of the ganglia to be confounded with the cineritious matter of the brain. However, this substance has also some peculiarities, which ought not to allow it to be confounded with the ordinary cellular membrane; but the nature of these is certainly unknown.

The idea, that the ganglia dispersed between the different branches of the sympathetic nerves, have the effect of protecting the filaments of the nerves, reserved for the organic life, from the influence of the animal life, ought to occur, and, in fact, did early occur to physiologists; but why have not the spinal ganglia, which resemble the others so much, the same effect? Of this point we are still ignorant: all is here involved in darkness and obscurity.

It is of no service to science, to offer any new opinion, or revive any old opinion, without having more proofs in support of the one than of the other. It is better openly to confess our ignorance, and to separate distinctly the things which are known from those which are not. The human mind, it is said, supports doubts with difficulty; but it is precisely on that account, that the learning to bear with them ought to be one of the principal studies of men of true learning. The wokrs of some modern physiologists have led us to this short digression.

The analogy of the cineritious cortical part of the brain, and cerebellum, with the tubercles in its internal substance, such as the corpora striata, the thalami optici, the nates, &c. is much better established than that of the ganglia. Every one recognizes their nearly identity of substance; sameness of function would there also be readily admitted. But what can we say of the comparison between it and the mucous body which lines the skin, and all its internal prolongations? Here there can only be, with respect to structure, texture, in short, physical nature, a resemblance purely hypothetical. In absence of actual observation, it would require, then, to justify this comparison, some resemblance in the functions, in the uses, in the mode of existence during life; but where shall we find these?

We confess, also, that we do not comprehend the relation between the masses of cineritious matter, where the medullary bundles are strengthened in passing through them, and the rings which surround the base of the young branches of a tree; in trees, the branches go off in sucession, one from the other; but in the nervous system, the whole is formed at once. It is impossible to find here any thing but an accidental resemblance.

Such, gentlemen, is the roport which we have thought it our duty to make to you.

We have repeated all the observations of Drs. Gall and Spurzheim; we have even submitted to a new examination, part of those which were made by older authors, but connected with theirs; in fine, we have pointed out the degree of justice which we have found in the old as well as in the new remarks.

We, therefore, think that we have fulfilled, as far as lay in our power, the commission with which the class has honoured us.

You will now perceive, that we are far from adopting all the views and all the observations related in the memoir of these anatomists, but that we are also far from rejecting them entirely.

Finally, it appears to us, 1st, That Drs. Gall and Spurzheim have the merit, not of having discovered, but of having recalled to the attention of physiologists, the continuity of the fibres which extend from the medula oblongata into the hemispheres, and into the cerebellum, which Vieussens first detailed, and the decussation of the filaments of the pyramidal eminences described by Mistichelli, by Francis Petit, and by Santorini, but with regard to which, some doubt had been entertained.

2d. That they are the first who have distinguished the two orders of fibres, of which the medullary matter of the hemispheres appears to be composed ; the one of which diverges from the pedunculi, while the other converges towards the commissures.

3. That, by uniting their observations with those of their predecessors, they have made it very probable, that the nerves called cerebral ascend from the medulla oblongata, and do not descend from the brain ; and that, in general, they have very much weakened, not to say overturned, the system which makes all the nerves come originally from the brain.

But it also appears to us, 1st, That they have generalized rather in an inconsiderate manner the resemblance of structure, and of functions in the various cineritious, or ash-coloured masses which are met with in the different parts of the nervous system.

2d. That the idea which they entertain of a solution of continuity in the middle of the medullary matter of each circumvolution, which would permit of its being unfoulded like a pipe or a purse, requires to be expressed in more definite terms than they have hitherto done, so as to indicate, that there is no complete proof of an absolute solution of continuity, but only of a more feeble cohesion.

We ought, however, to remark, that these two articles do not affect their general result, with regard to the kind of separation and of reserve into which they place the brain; and we ought, at the same time, to allow physiologists and pathologists to judge how far this kind of separation or laying aside, which anatomy seems to point out, is justified by facts, and how far it may favour the explanation of the numerous and astonishing phenomena of organic and animal life, and especially of those in which these seem sometimes dependent upon, sometimes distinct from each other.

The entering into all these questions would engage us in endless discussions, and foreign to our commission.

Nor shall we propose to the class to decide upon the conclusion drawn by our anatomists, that there is no circumscribed place in the brain to which all the sensations go, and from which all the voluntary motions issue, but that both of these functions may be exercised in a greater or smaller extent of the nervous system.

This opinion is undoubtedly that of Haller, Bonetus, and of the greater number of physiologists. There can be no boubt that it is from having confounded the metaphysical simplicity of the roul with the physical simplicity attributed to atoms, that the desire has originated of placing the seat of the soul in an atom; but the connection of the soul and body being necessarily incomprehensible to our minds, the more or less limited sphere which it is wished to give to the sensorium would not, in the smallest degree, assist our conception of it.

But all these subjects are also too foreign to the business of the class; they are too slightly connected with physical facts; they lead to discussions too vague to form a proper object of attention for such a body as ours.

We think ourselves, however, obliged to finish our investigation by remarking, that, if we were even to adopt the greater number of the ideas of Drs. Gall and Spurzheim, we would still be far from knowing the relations, the uses, and the connections of all the parts of the brain.

So long as we have not even a well grounded conjecture with regard to the functions of the pituitary gland, of the infundibulum, of the mammillary eminences, of the cords passing from these eminences into the substance of the thalami, of the pineal gland, and of its pedunculi, we must apprehend, that any system whatever, with regard to the functions of the brain, is very incomplete, since it will not embrace these parts so numerous, so considerable, and so intimately connected with the whole of this important organ.

This is almost finished with as much doubt, and as much uncertainty, as we began; but we can only require, on every subject, the degree of probability of which it admits; and the natural philosopher always performs his task very well, when he neither exaggerates, nor diminishes this probability, and when he fixes its degree with decision.

It is of importance to repeat once more, if it were only for the information of the public, that the anatomical questions with which we have been engaged in this report, have no immediate and necessary connection with the physiological doctrine taught by Dr. Gall, with regard to the functions and the influence of the relative size of the different parts of the brain, and that all that we have examined with respect to the structure of the encephalon might be equally true or false, without any conclusion being drawn for or against the doctrine, which can only be determined by quite different means.

Given at the Institute, the 15th April, 1808.

(Signed) Tenon, Portal, Sabatier, Pinel, Cuvier. The class approves of this report, and adopts its conclusions.

Conform to the extract,

(Signed) Cuvier, Perpetual Secretary.

Potter on the Anomalous Chraracter of the Measles of 1808.

It may justly be questioned whether indolence, or the fascination of great names, has contributed most to chain the minds of physicians to established systems; for such is their servile attachment to the prevailing systems of nosology, that it becomes the duty of every man of observation, to detect their fallacies and refute their errors. If in diseases which are said to arise from specific contagion, (for want of an elementary acquaintance with their remote causes) and which exhibit symptoms the most uniform, we find such manifest abervations from written laws, what are we to expect from such as are occasioned by the sensible qualities of the air, the vicissitudes of climate, and the innumerable remote causes that never cease to operate till death deprives the body of the capacity of being acted upon?

In a paper published in the Medical Repository, I noted some of the appearances of the measles of 1802, and marked some of their deviations from the ordinary symptoms of morbillous fever. As this epidemic observes nearly equidistant periods of visitation, appearing about once in six years, no occasion of further observation presented itself, till 1808; when it manifested characteristics, not only at variance with the preceding epidemic, but which sat at naught all the laws of nosological classification. Having observed, for many years, that such of our great epidemics as pervaded an extensive surface of our continent, generally commenced their ravages to the eastward of Maryland, and travelled westward, and having ascertained that the measles were prevailing on the western shore of the Delaware, I determined to watch their progress and note their phenomena. Although my success has not been complete, my observations may lead some future observer to a more satisfactory result.

Whether the measles prevailed in Jersey, previous to January 1808, I have not ascertained. The first intimation of their existence, was received from the opposite shore in the counties of Kent and Sussex, where they appeared early in January. About the middle of February, they began to appear sporadically in Caroline county, the eastern boundary of Maryland; and before the middle of April, they had pervaded the whole peninsula. It appeared to me that the cause travelled one uniform pace under all circumstances of situation, wetsher temperature or population, and that the general cause that produced them in the first instance, and not the extension of the disease by contagion regulated their movements. This disease reached Baltimore early in May, and continued to prevail till the middle of June; nor did it halt till its progress was arrested by the extreme heat of July and August, in the states of Virginia and North Carolina.

As the disease had appeared in 1802, it was to have been foreseen that it could not be generally epidemic, and that the children born since the last epidemic, would be the principal sufferers. This, however, was not the only cause, that diminished the numbers of sick. In many instances, only a solitary case occurred in a family where there were several children who had never been exposed to the causes. In one family only the eldest of ten children felt its influence; and in many not half the children were affected, although they slept in the same bed, and lived in all respects alike. Several persons who escaped in 1802, were now affected. Among these were some adults, who suffered much more severely than children.

The type of this fever, was not only different from that of 1802, but from that described by the European writers. It did not assume the decidedly inflammatory character, so common to measles, but maintained almost invariably, a typhoid type. The pulses were seldom hard or tense. The affection of the pulmonary system was scarcely to be remarked, and neither the notery eye nor coryza were often met with. The atatck was seldom preceded by a chill, and the alternate sensations of cold and heat were less remarkable than usual. After the first twenty four hours, the patient complained principally of a preternatural heat over the whole surface of the body and this sensation was often referred to the stomach, though in some cases this organ was entirely exempted from disease. A soreness of the throat was seldom complained of, and the raucedo voses so common in this disease, was scarcely perceptible. Neither pains of the head, back, or limbs, were much complained of. A dull heavy stupor reigned over the whole system, while a constant coma rendered the patient in a manner insensible to all passing events, except when the eyes were affected, as will be noticed in the sequel. The duration of the fever, as well as the period of eruption, constituted the most remarkable peculiarity of this disease. Instead of a continuance of the fever for a stated time, and the appearance of the eruption at the usual poriod, both were so irregular, that no rule could be established. Some patients laboured under a fever, for ten, twelve and even fifteen days, and convalesced without an eruption. In some the eruption appeared on the third or fourth day, but in others it did not appear till the seventh, tenth, eleventh, thirteenth, or fifteenth day, and in one case, an unremitting fever, continued till the twenty second day; when a copious eruption appeared, and the fever with the other symptoms, began to subside. Some of those who had suffered an attack of fever without the eruption were again attacked, and experienced a copious eruption. This was commonly after a lapse of twenty one days from the termination of the former attack. From this fact it may be questioned whether those who had the fever without the eruption, would be liable to the morbillous impression hereafter. Although some of them might have been rendered unsusceptible, it is most probable that the greater part of them, will be hereafter liable to the influence of the remote cause. The eruption seldom appeared complete in a few hours, or even in a day, but increased gradually from the slightest shade to a full eruption, proceeding by slow degrees, for five, seven, and sometimes ten days, before it was complete. In other cases it never was complete, but assumed an infinite variety of shades, from the faintest rubefacient subcuticular cloud, to the richest eruption. In a few cases, a pale crimson tinge appeared for a few hours, and vanished to be renewed again, at the intervals of three, five, nine, or eleven days, when it proceeded to a regulr crisis by a copious eruption. I ought to have semarked, that two persons were seldom attacked at the same time, in one family, but generally a second was taken sick on the twentieth or twenty first day from the indisposition of the first.

An hœmorrage from the nose sometimes preceded the fever, and generally on the day preceeding the attack; but in other cases, it immediately preceded the eruption. Although the eyes were not affected in the usual manner, they suffered in a very extraordinary degree. In many cases the affection of one of them, was coeval with the fever, and after continuing for ten or twelve days, the other become similarly affected, and the inflammation of the first, gradually subsided. In some cases, the affection of the organs of vision, survived all the other symptoms, assumed a chronic form, and continued for many weeks, and sometimes for two or three months. In one case, a chronic fever of the eye, continued six months. The whole globe of the eye was interested, and generally much swollen and protruded, in the early stage of the fever. The vessels of the adnata, were deeply suffused with red blood. In many cases after the subsidence of inflammation, an opagne cloud obfuscated the cornea, and obstructed the rays of light, altogether, but generally only partially. During every stage of this affection, such was the sensibility of the eye to light, that a dark room, or a covering of black, was indispensible. Even after the disease assumed a chronic form, a strong light immediately occasioned a copious secretion of tears, attended with acute pain.

The CURE of this disease was not attended with much difficulty. The type was so mild and the local affections so slight, that blood-letting was seldom required, unless it was indicated by the pain of the ball of the eye, and this was not the case in one of twenty of the number thus affected. Local bleeding by cupping was often very useful, and could it have been submitted to in every case where the eyes were concerned, would probably have been sufficient.—I regretted exceedingly the want of leeches, which promised every thing that could have been desired.—Blisters behind the ears were of essential service, especially where the discharge was kept up for several days.—As costiveness very frequently attended the inactive, recumbent state of the patient, gentle aperients become necessary. The sulphat of soda, or the acidulated tartrite of potash, were generally sufficient. As there were no increased morbid secretions of the stomach, intestines or liver, stronger cathartics were not required.—Emetics were of infinitely more use than in any other epidemic that has fallen under my notice. They were always useful as long as the heat of the stomach and skin were complained of, and generally afforded entire relief from these symptoms, although they seldom ejected from the stomach more than its natural secretion. Bile was seldom discharged, except after very frequently repeated or very violent efforts; and even then the portion was small, and its appearance natural. Emetics answered another valuable purpose. In cases where the eruption was retarded till after the usual time of appearance, they very often produced it in an hour or two after they ceased to operate, and put a period to the anxiety and restlessness that had been so distressing. This remedy did not occur to me in the beginning of the epidemic, but experience ere long taught me to rely on it as a safe and excellent practice.—It ought not to be omitted, that when the affections of the eyes remained after the patient had recovered from the other symptoms, small and long continued doses of the sub muriate of mercury generally succeeded in removing them, and in many cases without the least affection of the salivary glands.—The only remaining remedy worthy of notice, is the application of *cold*. I had been, very early in the disease, in the habit of bathing the eyes in cold water, sometimes in the form of ice, and with the happiest effects; but had not attended sufficiently to the temperature of the apartment. Although the cool regimen had all along been observed, the salutary effects of a lower temperature were not duly estimated, till Dr. Stevenson* called my attention to it. He informed me, "that the lowest degree that could be obtained at that season, was not only the most agreeable to the patient, but a most powerful mode of destroying the heat of the body so much complained of." During the first week of May, while the mercury of Farenheit varied from 50 to 55° in the shade, the windows were frequently thrown up, and the external air suffered to equalize the temperature of the room with that of the external air. The patient always expressed the greatest satisfaction, and the most striking proof of its utility was manifested by the removal of the coma and general stupor that attended all the strongest marked cases. The face and hands were often bathed with cold water, to the inexpressible delight of the sufferer. Of the great number of cases that fell to my lot, not one proved fatal.

---

* In a paper published in Barton's Journal, and the Baltimore Recorder, Dr. Stevenson insisted on the propriety and utility of the *cold* regimen; but many having considered it only as an encomium on the old doctrine of a cool regimen, have not been aware of its importance.

REVIEW.

An Inaugural Dissertation on the Medical Virtues of the White Oxyde of Bismuth.

By Samuel W. Moore, A. B. New-York, Printed by T. & J. Swords.

> "——omnes boni medici ad ea remedia semper confugient, (sive
> " modus quo prosint intellectus fuerit necne) quæ certa expe-
> " rientia prodesse comprobaverit."
>
> *Greg. Consp. Med. Theoret.*

ALTHOUGH the Magitery of Bismuth was introduced into practice by Dr. Odier of Geneva, in 1786, like opium, mercury, arsenic and every other of the most energetic remedies, it has been alternately neglected and revived by the practitioners of Europe. The volume of evidence in its favor, is however, now so full, that the proofs of its utility can be no longer resisted. Dr. Odier stated it to have been found in his hands " a powerful remedy in spasmodic pains of the stomach and bowels, particularly such as arose from organic debility or a relaxed and emaciated constitution."

Marcelot in his " Cours Elimentaire Theorique et pratique Pharmacie Chemique in 1803, after speaking of it aa a mineral but little used in medicine adds, " L'oxide de bismuth est employe, interieurement par qeulques praticiens, a la dose de 4 a 6 decigrammes, soit in pilules soit en poudre dans une boisson appropriee, dans les engorgmens lymphatiques."

Dr. Willich in his Domestic Encyclopedia remarks, " in medicine the calx and flowers of bismuth, were formerly used in cases where antimonial preparations are now employed with greater safety, and equal effect : so that the former are at present chiefly converted into pigments and cosmetics. Nevertheless, we are possessed of the most convincing proofs, that the magistery of bismuth is one of the most powerful antispasmodics, especially in cramps of the stomach.

Dr. Marcet in his paper read before the London Medical Society, in 1801, but not published till 1804, states in a note " since the above paper was read before the society, I have had sufficient opportunities at Guy's Hospital, of trying the oxyde of bismuth, in spasmodic affections of the stomach, and those trials have fully confirmed the opinion which I gave three years ago on the utility of this medicine."

The Edinburgh Medical and Surgical Journal for October 1806, in the report of the Carey-street Dispensary, gives the following paragraph : " For the gastrodynia which is extremely frequent among the poor at all seasons, the oxyde of bismuth, which was recommended by Dr. Odier, of Geneva, has been employed both by my colleague Dr. Laird, and myself with considerable success. It has in some cases produced a permanent relief, after the usual tonics and stimulants had been taken with a trifling and but temporary advantage."

The London Medcial and Physical Journal for 1806, speaks in high terms of commendation of this medicine, " in dyspepsia, and views it is an important addition to the list of stomachics."

In the Journal de Medicine, Avril 1807, Mons Leroux extols the power of the oxide of bismuth, in affections of the stomach, and many other diseases, but does not specify the nature or characters of the other morbid states in which he had found it useful. He adds Mais jai ete oblige d'en porter le dose beaucoup plus haut qu'il n'est indiquè dans l'article qu'on vient de lire. J'en ai quelquefois donne chaque *jour jusq'a trente groins divises en trois doscs et je n'en jamais ivu resulter d'inconvenient.*"

Dr. Clark, in the Medical Report for Nottingham, published in the Edinburgh Medical and Surgical Journal, in 1808 and 1809, corroborates the preceding accounts by the most decided evidences of the virtues of the oxyde of bismuth.

Notwithstanding the mass of testimony from abroad in favor of this new addition to our pharmacopeias, it seems to have attracted but little of the attention of the faculty of the United States, till Dr. Moore reduced the scattered notices of its powers into one body in his dissertation. We abstract from this judicious essay as much of Dr. Moore's experience and reasoning, as the limits of our plan will admit.

New-York, August 1, 1810.

Case 1st. C. a mulatto woman, aged thirty-six, has, for the last seven months, been dyspeptic, during which time I have occasionally visited her and given her medicine; she is better, but still complains of being troubled every morning about four o'clock, while still in bed with sickness, preceded generally by coughing a few times, when she throws up from her stomach a watery fluid, seldom very acid. She assures me she has not been free from these attacks, scarcely for a single day during the last seven months. She has more than once taken emetics, and succeeded their use by gentian and orange-peel, without deriving from them much benefit.

Although I did not doubt that many cases might be found in which bismuth was calculated to do more good than in the present one; I nevertheless felt a desire to give it a trial, and accordingly directed her to take, three times a day, five grains of the oxyde,* with fifteen of gum-arabic and sugar.

September 5th. She is better, and tells me, she vomited but twice since she commenced taking the powders. She has taken seventeen doses of the powders.

---

* *Dr. Moore's mode of preparing the White Oxyde of Bismuth.*—The bismuth to be dissolved, should be previously reduced to powder in an iron mortar. Let three parts of nitric acid for one of bismuth be diluted with an equal weight of pure water. To this menstruum contained in a glass vessel, add the bismuth at intervals, and let it stand till it is all dissolved. Let the clear solution be decanted from the sediment, (a small quantity of which will always be found) and a few ounces of it be poured into a glass vessel capable of containing half as many gallons as there have been measured ounces put in; the vessel is then to be filled with pure water, when a copious and perfectly white precipitate will be instantaneously formed, giving to the liquid the appearance of milk.

9th. She has very little sickness, but still throws up in the morning a small quantity of fluid from her stomach. I gave her to-day some more of the oxyde, with directions to take it as before.

12th. Since she recommenced the use of the remedy, she has had no return of either sickness or vomiting.

New-York, September 9, 1810.

Case 2d. Mr. L. aged forty-five, has, for the last three years, been afflicted with cardialgia, attended by extreme flatulence; he has at no time during this period, been free from these complaints. He has frequent acid eructations,

---

After this has subsided, the clear fluid must be decanted, and fresh water thrown on the precipitate to wash it. This operation must be several times repeated, until no acid taste is discernible in the decanted water.

This precipitate, which is the true white oxyde of bismuth, should be suffered to dry without heat, or indeed light, for the attraction between bismuth and oxygene is so weak, that if the oxyde, while drying, be exposed either to a very moderate artificial heat, or the direct rays of the sun, it parts with a portion of its oxygene, and loses its whiteness.

The oxyde prepared in this way, is purer and whiter than that formed by any other process. I have precipitated it from its solution, both by potash and ammonia, but in neither case was it so pure a white; and indeed the administration of the oxyde, prepared by means of an alkali, might be attended with danger, in case the bismuth had been alloyed with other metals, which would also be thrown down by the addition of an alkali to the solution.

Whatever reason we may have for thinking the bismuth we make use of impure, (which can scarcely be the case without being discoverable from its appearance) still we have no reason to fear, that the oxyde will partake of the impurities of the metal which has been employed, if it be prepared in the manner which has been directed; for Nicholson assures us, "the precipitation of the nitrous solution by the addition of water, is the criterion by which bismuth is distinguished from all other metals.† And indeed, if our only object were to separate the bismuth from its alloys in a state of purity, no better means could be employed to obtain our end, than those which have been recommended for preparing the oxyde.‡

† Nicholson's Chem. Dict.

‡ No glazed earthen vessel should be employed, on account of the lead so frequently used in glazing them.

which he says are also very acrid, occasioning his throat to feel as though it had been scalded; sometimes also, though rarely, he is sick at his stomach when he vomits up a matter having the color of coffee-grounds. He has a number of times taken emetics, and since the commencement of his complaint, has been in the daily habit of taking magnesia; from the use of which, although he has sometimes thought it afforded him temporary relief, it is pretty evident, from the continuance of his disease, that very little permanent advantage has resulted.

During the last winter and spring, Mr. L. had occasional attacks of some gnawing pains at the pit of his stomach, which warm external applications would commonly in the course of fifteen or twenty minutes relieve. Throughout the summer, he has been generally free from those spasmodic pains; but for the last eight or ten days, they have attacked him much more severely, usually returning two or three times a day. These attacks are very violent, occasioning great anxiety, succeeded by insufferable pain, throwing him immediately into a profuse perspiration. The warm applications, from the use of which he had on former occasions found benefit, at this time afford him not the slightest relief.

For the last two months, he has been making use of very strong bitters, and I think it possible he has injured his stomach by the large and repeated doses he has taken. A very sparing use of either fermented or spirituous liquors never fails to increase his cordialgia. His bowels are quite regular and his pulse feeble.

Mr. L. sent for my father, who directed him to take three times a day, five grains of the oxyde of bismuth, combined with an equal weight of refined sugar, and twice its weight of starch.

September 11th. He has had no return of the pain in his stomach since he commenced the use of the oxyde, but feels well enough to-day to take a ride into the country.

Sept. 17th. He still continues free from the spasmodic pain of his stomach, his cardialgia, although not well, is better than it has been hitherto. He has taken none of the bismuth since the morning of the 13th, when he took the twelfth and only remaining dose he had, and being out of town, he had it not in his power to procure another supply until his return to the city to-day.

Monday evening. He had this afternoon a return of that distressing anxiety which has always hitherto preceded his attacks of pain in the stomach. Fearing he was about to have another attack, and placing full reliance on the oxyde from his experience of its efficacy, he sent immediately for a supply of it; but before the person returned with the medicine, he had taken forty drops of laudanum; he however took directly a dose of the oxyde, and the pain did not, as he expected, succeed; but as he had taken laudanum previously, we should not be satisfied in attributing to the bismuth alone, the disappearance of his unpleasant symptoms.

Wednesday, Sept. 19th. Our patient has not since had any return of the spasm; the cardialgia is infinitely less troublesome. I gave him to-day, twenty-four doses of the powder, each containing six grains of the oxyde, directing him to take one three times a day.

Sunday evening, Sept. 23d. Mr. L. is now free from all complaint. His heart-burn, which was very troublesome till he began taking the bismuth, does not at present appear, unless it be brought on by the use of wine or brandy; he can however drink moderately of West-India spirits or gin, without experiencing the least inconvenience from so doing. Thursday last, after riding twelve or fourteen miles, he began to feel some pain in his stomach, but was immediately relieved by taking his customary dose of the oxyde.

New-York, Sept. 24, 1810.

Case 3d. Mr. B. the subject of the following case, has been for three or four years dyspeptic; during which time, he has almost continually suffered from cardialgia and pyrosis, and very frequently severe colic pains. The fluid which he threw up from his stomach, was, at times, he said, so extremely acid, that it felt as if it were cutting his throat while passing through it. He has been long in the habit of using the alkaline solutions, absorbents, &c. but without effect.

Mr. B. has, at my request, very obligingly drawn up a statement of his own case, the substance of which I shall give nearly in his own words :

I have been for several years troubled with acidity in my stomach, a watery fluid running from my mouth, and an unpleasant sensation or pain at my breast, frequent colic pains ; sometimes a cough, which was always accompanied by an unpleasant taste ; a great heaviness and inclination to sleep, with an aversion to motion. My appetite has always been good, and sometimes indeed voracious ; but even at these times my stomach would be often disgusted by the first mouthful of food I swallowed. When the heaviness came over me, I felt a sensible diminution of my strength and spirits. I have taken soda, magnesia, chalk, and rhubarb; but have found no relief from them. On the 16th of Sept. I called on Dr. Moore, and obtained from him a few powders, of which I have taken three a day, and have not, for the last five days, felt any symptoms of the complaint.

Sept. 24th, 1810.

Mr. B. took fourteen doses of those powders, containing altogether one dram and ten grains of the oxyde. He assures me, that he has never felt better than he does at present, and says, that if the powders produce a permanent cure, it will be one of the happiest events of his life.

Sept. 20th. I saw our patient this day, when he informed me of his having continued thus far perfectly free from all indisposition."

Dr. Moore adds, that " Dr. Stringham, of New-York, has lately administered the oxyde with astonishing relief in a case of menorhagia."

These facts are sufficient to evince the safety and utility of the internal use of the oxyde of bismuth, and leave us but one point for doubt or apprehension. The tone of the stomach once impaired, is so predisposed to a repetition of the same morbid phenomena, that notwithstanding the temporary relief derived from various stimuli, relapses are very common; but we trust, that the candor of Dr. Moore and the experience of the faculty, will soon determine whether the oxyde of bismuth be a radical or palliative remedy.

We tender our acknowledgments to Dr. Moore, for the pleasure and instruction we have derived from his ingenuity and industry. We consider his dissertation as a plain, candid and discriminating exposition of his subject; very remarkable for that "lucidus ordo" preserved throughout, which falls to the lot of very few juvenile writers.

"Premordia vestra
Vix pauci meruere senes: metasque tonetis,
Ante, genas dulces quam flos juvenilis inumbret
Oraque ridenti lanugine vestiat ætas."

Dr. Daniel Drake, in a pamphlet modestly entitled "Notices of Cincinnati," has exhibited an accurate account of the Topography, Geology and Climate of this delightfu region. To these he has added, a Celendarium Floræ of the country. He promises us also "memoranda of diseases," from which we anticipate a rich repast. He concludes his researches, with the following description of what he has denominated, "A new disease," and which we are disposed to consider as one of the numerous family that are to be referred to miasmata for their remote causes.

In the spring of 1809, Dr. Barbee, of Virginia, on returning from a visit to the Madriver country, in this state, gave me some information respecting a new and formidable disease which had appeared among the settlers of that tract. Since that time, I have been able to collect several additional facts respecting it, from different persons, more especially Mr. William Snodgrass, and Mr. John M'Kag, two intelligent and respectable inhabitants of that country, who have several times experienced the disease in their persons and families. A summary of the whole, is here given, that physicians may determine how far it deserves the appellation of a new disease.

It almost invariably commences with general weakness, and lassitude, which increass in the most gradual manner. About the same time, or soon after, a dull pain, or rather soreness, begins to affect the calves of the legs, occasionally extending up to the thighs. The appetite becomes rather impaired, and in some cases nearly suspended ; sensations of a disagreeable kind affecting the stomach : upon taking a little food, however, a greater disposition for it is generated, and more agreeable feelings are introduced throughout the whole system. Intestinal constipation in this, as in all the subsequent periods of the disease, exists in a very high degree. A strong propensity to sleep occurs, and according to Dr. Barbee, the pulse " is full, frequent, round, and *somewhat* tense, but regular." During this stage, exercise of any kind is highly detrimental, and if persisted in, soon produces loathing and nausea at the stomach. If the patient repose, upon first experiencing these symptoms, they generally cease, and he is allowed a longer exemption from the *vomiting* that awaits him. Sooner or later, however, that symptom almost invariably succeeds the predisposition we have described, and either proves fatal in one, two, three, or more days, or leaves the patient in a most exhausted state, from which he recovers only to sustain, at no distant period, a repetition of the same attack.

The matter ejected is sometimes bilious, but much oftener sour, and so acrid, that its action on the throat, in one case, (which proved fatal) was likened to boiling water. Towards the close of mortal cases, it is occasionally very dark coloured, so that it has been compared to that very convenient and fashionable object of similitude—coffee-grounds. At this time the intestinal constipation is very great: Mr. Snodgrass knew one patient in whom it continued for nine days, throughout which he took no food whatever, and vomited during six of them. After such an attack, the propensity for sleep is destroyed, and an uncommon degree of watchfulness is produced. The patient remains languid, and his face and person generally become rather tumid. His skin is cool, palish, and frequently affected with clamminess. He has a disagreeable burning sensation in his stomach, and hot eructations are very troublesome. The thirst is considerable. The breath is peculiarly disgusting, even loathsome. The appetite is generally poor; and the inclination to costiveness remains. These symptoms often continue for several months, during which the patient experiences frequent returns of the vomiting. But at length, more especially upon the approach of winter, they gradually wear away, leaving the patient considerably worse than they found him, and liable to fresh attack the ensuing summer.

Nothing like *regular* periodical exascerbations are observable in this disease; no chilliness occurs; the colour of the skin and eyes do not deviate widely from that of health, and give no striking indication of bile ; there is no pain in the region of the liver, nor in the shoulder ; it does not terminate in dropsy ; nor are there any symptoms which bespeak it a disguised or anomalous intermittent. It however prevails (though not exclusively) in aguish situations, and intermitting diseases are thought to have declined since its appearance.

It affects all ages, conditions, and both sexes, indiscriminately ; except probably very young children. They however are not wholly exempt from it. Emigrants are not peculiarly liable to it. It was first observed in the summer of 1806, and is thought annually to extend its geographical range, and to become more intense. It sometimes commences in July or before, but oftener in August, and continues till the approach of winter, when it generally, but not always subsides.

The cure of this disease seems hitherto to have been left chiefly to the people, who have not yet discovered any certain method. Purging was a remedy that naturaly suggested itself ; and by some it had been thought very serviceable, more especially when effected by aloes ; but others assert that they have frequently known a cathartic to increase the vomiting, and therefore rely more on enemata. All agree however, that the intestinal obstructions are to be overcome ; and that the less the means made use of, affect the stomach, the better. Vomits evidently do harm. Blisters to the gastric region are considered the most efficient remedy, tonics have been used, but no great benefit appeared to arise from them. Wine and salted meats, however, have appeared to do good, and are relished beyond any thing else. Indeed, eating a little frequently, whether an inclination exist or not, has been found a good palliative : It relieves the stomach from the gnawing which so perpetually exists. Alkaline lee has been used in one case : it gave some temporary relief, but not more than almost any other substance which might be received into the stomach. Bleeding has been occasionally resorted to, but with doubtful advantage. Ardent spirit appears to render the disease worse: It is not however, much sought after, all inclination for it, generally being destroyed. Tea and coffee, also, with several other articles of diet, which were agreeable before the disease, are in many cases disliked for a long time after.

This disease is unequivocally observed to effect four domestic animals; the horse, the cow, the sheep, and the dog. It is often fatal to the two former; but not so fatal to the latter. It as frequently attacks horses in the winter as summer, and sometimes kills them in twenty four hours.

It prevails chiefly in the neighbourhood of Staunton, on the great Miami, and in the country south of Madriver, between Dayton and Springfield. In those tracts, ponds, and marches occasionally occur, more especially in the former. The soil and water are calcareous. The timber generally oak.

At a commencement in the University of Pennsylvania, held on the 26*th of April*, 1811, *the Degree of Doctor of Medicine, was conferred on the following gentlemen; citizens of the state of Maryland.*

John Austin, John H. Briscoe, William Hammond, John D. Perkins, William Purnell, Robert J. H. Purnell, John D. Readel, James Stewart, and Matthew Vanlear.

ERRATA.—Page 24, fourth line from the bottom, a *comma* instead of a *period*, after the word *application*,

Page 83, tenth line from the top, instead of *observations*, read *aberrations*.

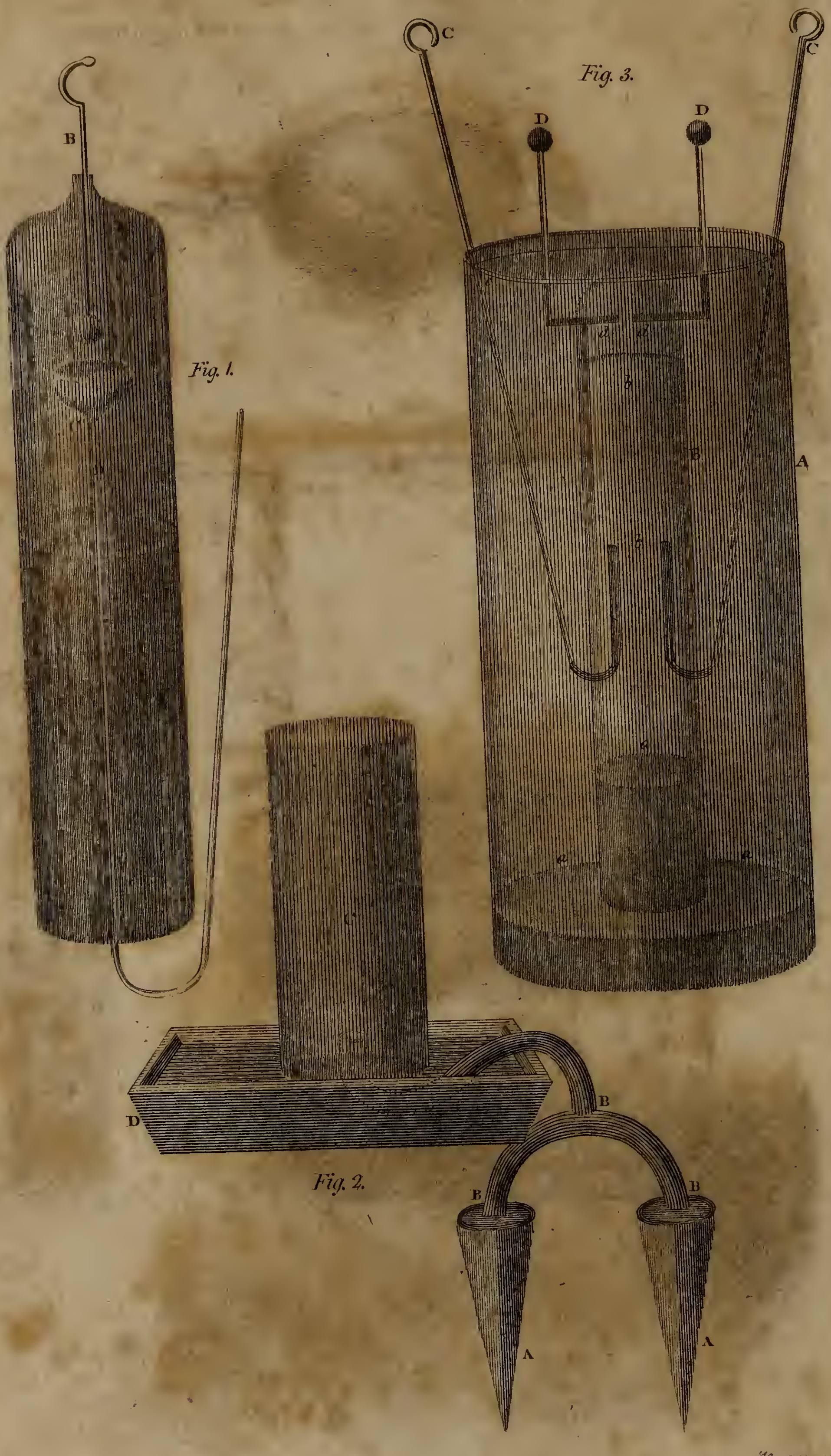
Page 85, seventh line from the top, instead of *voses*, read *vocis*.

Page 89, third line of the review, for *every*, read *many*.

Page 96, fourth line from the bottom, instead of *premordia*, read *primordia*.

Page 89, first line of the subject, instead of *Magitery*, read *Magistery*.

Professor Davy's electro-chemical apparatus



J. Kneass sc.

Professor Davy's electro-chemical apparatus.

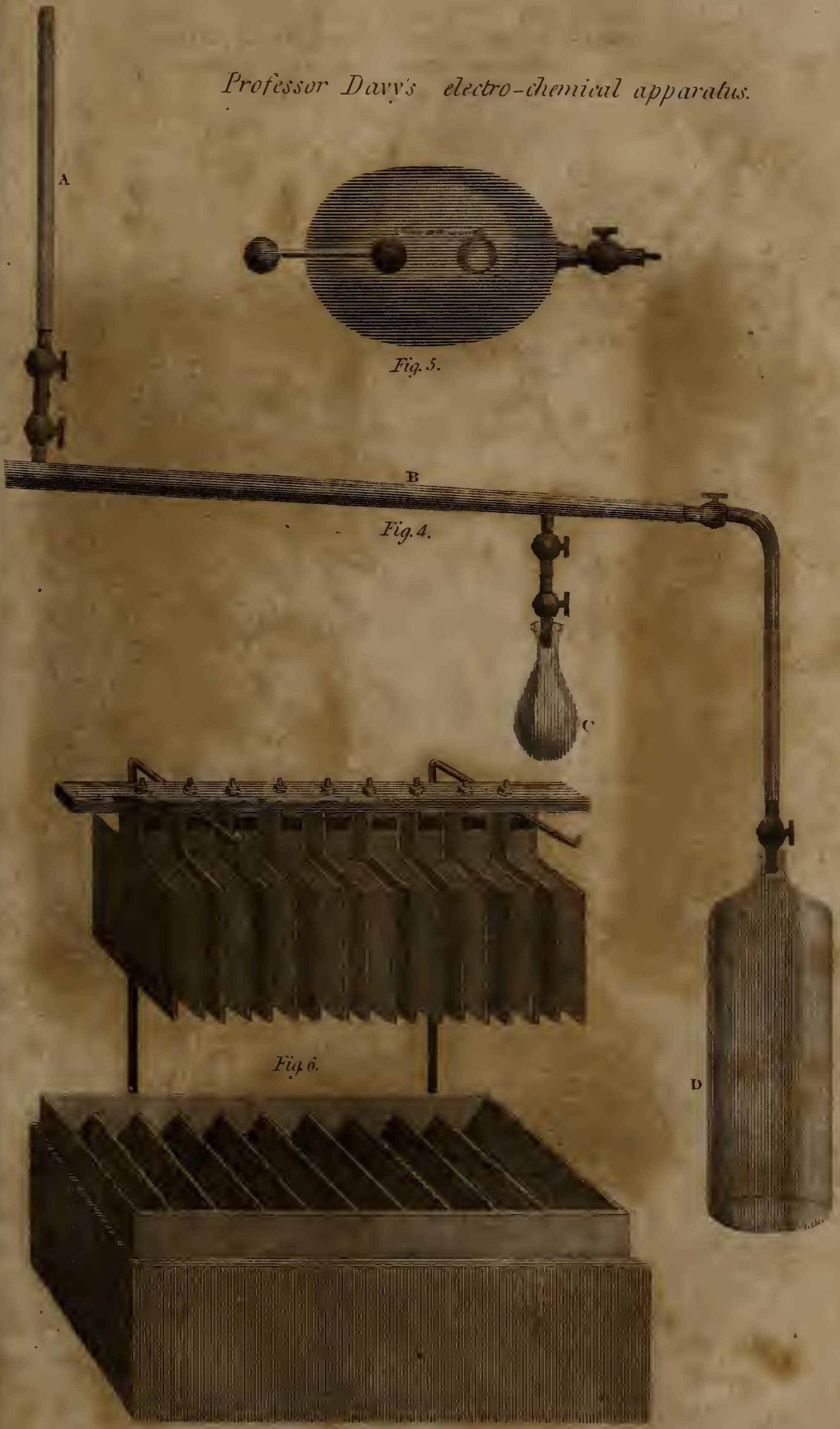


Fig. 5.

Fig. 4.

Fig. 6.

[illegible] sc.

THE

BALTIMORE

Medical and Philosophical Lycæum.

VOL. I....NO. II.

FOR APRIL, MAY AND JUNE.....1811.

The Bakerian Lecture for 1809.

On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.

BY HUMPHREY DAVY, Esq. Sec. R. S. F. R. S. E. M. R. I. A.

From the Philosophical Magazine.

1. Introduction.

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries on the application of Electricity to Chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views, and applications, not unconnected with the objects of the Bakerian Lecture: and though many of them are far from having attained that precision, and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

II. *Some new Experiments on the Metals from the fixed Alkalies.*

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phenomena.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of enquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phenomena, different from those I adopted.

M. M. Gay Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda with hydrogen; a similar opinion seems to be entertained by M. Ritter. M. Curaudau* affects to consider them as combinations of charcoal, or of charcoal and hydrogen, with the alkalies; and an Inquirer† in our own country regards them as composed of *oxygen* and *hydrogen*.

I shall examine such of those notions only as have been connected with experiments, and I shall not occupy the time of the society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which M. M. Gay Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur; and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mem. d'Arcueil, a copy of which M. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail, it seems that they still retain their opinion; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the society, I shall venture to state fully their method of operation, and of reasoning.

---

* *Journal de Physique*, June 1808.

† Nicholson's Journal, August 1809, p. 258.

They say that they heated potassium* in ammonia, and they found that a considerable quantity of ammonia was absorbed, and hydrogen produced; and that the potassium became converted into an olive-coloured fusible substance; by heating this substance strongly, they obtained three-fifths of the ammonia again, two-fifths as ammonia, one-fifth as hydrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two-fifths, and found in the vessel in which the operation was carried on, nothing but potash.—Again, it is stated that by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm that they can procure from a given quantity of ammonia, more than its volume of hydrogen.

Whence, they ask, can the hydrogen proceed? Shall it be admitted that it is from the ammonia? But this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. Berthollet, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogen gas must be produced from the metal;

---

* *Mem. d'Arcueil*, tom. ii. p. 309.

and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogen, and that alkali.

It is obvious, that even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxygen which this would give to the potassium, and of the hydrogen which it might furnish, to reproduce the ammonia, they take no notice.

I have shown by numerous experiments, many of which have been repeated before members of this society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion is less ammonia regenerated; and I have seldom obtained as much as one tenth of the quantity absorbed. And I have never procured hydrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions, [Phil. Mag. vol. xxxiv. p. 344.] and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from three to four grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in these cases there was a considerable deficiency of hydrogen, as well as nitrogen, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases I obtained more than four-fifths of the potassium which had been employed; and very nearly the whole of the nitrogen, existing in the ammonia that had been acted upon.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at $30.2^{in.}$ thermometer at 54° Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7.5 were absorbed, and 3.2 of hydrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors, was exhausted, and filled with hydrogen. They contained together eight-tenths of a cubical inch. The heat was very slowly applied by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and one half of a cubical inch remained in the retort and adaptors. Of the nine cubical inches, one fifth of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxygen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed, was such as to generate by its action upon water three cubical inches and three-tenths of hydrogen gas.

Now if this experiment be calculated upon, it will be found, that 7·5—·2= to 7·3 of ammonia, and its electrical decomposition, would afford about 13·1 of permanent gas, containing about 3·4 of nitrogen, and 9·7 of hydrogen. But the 3·2 cubical inches of hydrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the process, =9; and the nitrogen in the 8·8 cubical inches of gas (or the 9—·2 of ammonia) will be about three, and if we estimate ·34 of hydrogen, and ·16 of nitrogen, in the ·5, remaining in the retort, there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the eight-tenths of hydrogen pre-existing in the tube and adaptors, the loss of hydrogen will be found proportionally rather greater than that of nitrogen.

In another experiment in which three grains of potassium were employed in the same manner, 6·78 cubical inches of ammonia were found to be absorbed, and 2·48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors, the asme quantity of residual air as in the process last described.

The 8 cubical inches of gas contained scarcely two-fifths of a cubical inch of ammonia; and the unabsorbable part detonated with oxygen, in the proportion of 11 to 6, gave a residuum of 7·5.—The barometer was at 30·2$^{\text{in}}$, thermometor at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys, were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6·78—· of ammonia=6·38, and this quantity of gas decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hydrogen; but there are produced in this experiment of hydrogen, 2·48 in the first operation, and 4·28 in the second, and considering the nitrogen in the permanent gas as 3·32, ·8 must be subtracted for the common air; which would give 2·53 for the nitrogen generated: and to these must be added, the quantity of hydrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hydrogen.

In all experiments of this kind, a considerable quantity of black matter separated during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort containing oxygen gas; there was a diminution of the gas; and a small quantity of moisture condensed on the upper part of the retort which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from three to four grains of potassium were made to act upon ammonia in a vessel of platina, and afterwards distilled in contact with platina, there were always from four to six grains of this powder formed; but I have advanced no farther in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter which affords water by combustion in oxygen.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last bakerian lecture, and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hydrogen. When copper tubes are employed, the hydrogen bears a smaller proportion to the nitrogen, and more potassium is revived.

In these experiments, in which platina has been used, there is little or no loss of potassium of nitrogen; but a loss smaller or greater of hydrogen.

It will be asked, on what do these circumstances depend? Does the affinity of certain metals for potassium prevent it from gaining oxygen from ammonia, and do platina and copper combine with a small quantity of hydrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not *a metallic* body that is decompounded but the volatile alkali, and that the *hydrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygen and hydrogen.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

$3\cdot\frac{3}{10}$ grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hydrogen, and the fusible substance which was very similar to that from potassium, distilled, did not give off $\frac{1}{20}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced, equalled twelve cubical inches, and, by detonation with oxygen, proved to consist of nearly two of hydrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention, the mere visible phenomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal, which is decompounded in this process.

As sodium does not act so violently upon oxygen, as potassium; and as soda does not absorb water from the atmosphere with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green, it becomes quietly and silently converted into the fusible substance which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form, evidently by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogen.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. Curaudau, that the metals of the alkalies are composed of the *alkalies* merely united to charcoal, the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which M. M. Gay Lussac and Thenard have been misled by the moisture which interfered with their results.

M. Curaudau states, that when sodium is oxidated, carbonic acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in eight cubical inches of oxygen: nearly two cubical inches of oxygen were absorbed, and soda in a state of extreme dryness, so that it could not be liquefied by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phenomena which I have described in the Bakerian lecture for 1807. Nearly six cubical inches of hydrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignated iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination; and it appears from M. Curaudau's method of decomposing the alkalies, that his metals must have been carburets, not of potash and soda, but of potassium and sodium.

M. Ritter's argument, in favor of potassium and sodium being compounds of hydrogen, is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxygen than potassium, and on the hypothesis of hydrogenation, must contain much more hydrogen; yet though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly,* yet sodium is heavier than potassium in the proportion of 9 to 7, at least.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxygen than sodium; and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

M. Ritter has stated, that of all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxygen is given off at the positive surface, no hydrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogen than that alkali.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find that instead of proving that potassium is a compound of potash and hydrogen, they confirm the idea of its being as yet like other metals undecompounded.

---

* *Hassenfratz, Annal. de Chim.* tome xxviii. p. 11.

When tellurium is made the positive surface in water, oxygen is given off; when it is made the negative surface, the Voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opaque and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hydrogen in water; which, in being diffused, is acted upon by the oxygen of the common air, dissolved in the water, and gradually loses a part of its hydrogen, and becomes a solid hydruret of tellurium. The compound of hydrogen and tellurium produced at the negative pole when uncombined is gaseous at common temperatures, and when muriatic acid or sulphuric acid are present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large Voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found annexed to this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which when touched by water, did not inflame nor effervesce, but rendered the water of a beautiful purple colour, and when thrown into water, entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hydrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium, and that the *telluretted* hydrogen (if the name may be used) had formed with the oxidated potassium, *i. e.* the potash, a peculiar compound, soluble in water; and this I found to be the case; for on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hydrogen; metallic tellurium was formed where it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was further confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogen was evolved by the action of the alloy upon water; but the compound of telluretted hydrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other, induced me to conceive that the *decomposition of potash* might be easily effected, by acting on the oxide of tellurium and potash at the same time, by heated charcoal; and I soon proved that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hachett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium, I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aeriform* compound of tellurium and hydrogen; I shall mention merely some of its most remarkable qualities and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hydrogen is more analagous to sulphuretted hydrogen, than to any other body. The smell of the two substances is almost precisely the same.* Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffiusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic, but which is soon converted into muriate of tellurium.†

---

* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children, Esq. of Tunbridgé, in which Mr. Children, Mr. Pepys, and Mr. Warburton co-operated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of Voltaic electricity, or by potassium; and some researches made upon the habitudes of different metallic sulphurets at the Voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hydrogen in the Voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxygenation could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and further researches, made in the laboratory of the Royal Institution, proved that the substance in question was a new and singular combination.

† From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogen, en-

As arsenic has an affinity for hydrogen, it occurred to me as probable, that it would present some phenomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark-coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogen by the action of water with inflammation, and deposited a brown powder.

When potassium and arsenic* were heated together in hydrogen gas, they combined with such violence as to produce the phenomena of inflammation, and an alloy was produced of the same kind as that formed by means of the Voltaic battery.

As tellurium and arsenic both combine with hydrogen, it appeared to me probable, that by the action of alloys of po-

---

ters into combination with it. An accident prevented me from ascertaining whether the compound so formed, is exactly the same as that described in the text.

* In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, Fourcroy Connais Chem. tom. viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetitiou of the process I find, that though potash is decompounded in this operation, yet that the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogen. The gases not absorbable by water given off in this operation, are peculiar. Their smell is intensely fetid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogen: whether they are mixtures of various gases, or a single compound, I am not at present able to decide.

tassium, with tellurium and arsenic, upon ammonia, some new phenomena would be obtained, and probably, still further proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendor, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four-sixths nitrogen, instead of being pure hydrogen, as in the case of the action of potassium alone.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas which was principally nitrogen; so that if it be said that the metal, and not the volatile alkali, is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hydrogen; which are contradictory assumptions.

None of the chymists who have speculated upon the *imaginary hydrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in which agents, which they did not suspect, were concerned. No person, I believe, has attempted to show that when potassium or sodium is burnt in oxygen gas, water is formed, or that water is generated when potassium decomposes any of the acids;* and no one has been able to form potassium, by combining hydrogen with potash. I stated in the Bakerian lecture for 1807, that when potassium and sodium were burnt in oxgen gas, *the pure* alkalies were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts of oxygen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find that this is the case; for the results differ very little in some experiments which I have made upon considerable portions of potassium and sodium, procured by chemical decomposition.

---

* When in October, 1807, I obtained a dark-coloured combustible substance from boracic acid, at the negative pole in the Voltaic circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution, March 12. Soon after, I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. M.M. Gay Lussac and Thenard have likewise operated

When potassium is burnt in trays of platina, in oxygen gas that has been dried by ignited potash, the absorption of oxygen is about $\frac{11}{20}$ of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain.* The alkalies so formed, are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'Arcet has shown by some very well conducted inquiries, that potash and soda,† in their common state, contain a considerable proportion of water; and M. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute ex-

---

upon boracic acid, by potassium, and they conclude that they have decompounded it; but this does not follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash: the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hydruret of boracic acid.

* The quantities of gas given out by the operation of water, are in a similar ratio. See page 43, of the last Bakerian lecture, [Phil. Mag. vol. xxxiii. p. 432.] and page 26 of this paper [p. 136 and 137 preceding.]

† *Annales de Chimie*, Nov. 1808, page 175.

periments, infers, that muriate of potash, which has been ignited, contains in the 100 parts 66·66 potash, and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances, I obtained from ten parts of potassium 17·5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner: five grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light;* and the whole mass appeared in igneous fusion; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about five of hydrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

The second experiment was conducted with still more attention to minuteness. Eight grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phenomena as in the last experiment, and the increase of weight of the tray was $6\frac{1}{2}$ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

---

* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1·4 grains of oxygen, to form 9·4 grains of potash, and 6.6—1·4—5·2, the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash; but 35.6 of muriatic acid, according to M. Berthollet's estimation, would demand 71·1 of alkali, in the state of dryness in which it exists in muriate of potash, and 71·1—64·4—6·7—so that the potash taken as a standard by M. Berthollet, contains at least 9 per cent. more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali.*

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

---

* Consequently M. Berthollet's fused potash must contain nearly 23 per cent. of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium as the dry standard.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides: this part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

One of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia, was, that nitrogen might possibly consist of oxygen and hydrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chymists from pursuing the same paths of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists, on the origin of nitrogen, produced during the passage of water through red-hot tubes, or the speculations of Girtanner, founded on these and other erroneous data: the early discovery of Priestly on the passage of gases through red-hot tubes of earthen ware, the accurate researches of Berthollet, and the experiments of Bouillon La Grange, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity.* To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxygen and hydrogen with great rapidity, and in which these gases could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity that about the eighth of a cubical inch of the mixed gases, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about a half a cubic inch; it had been carefully purged of air by the air pump, and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygen and hydrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gases; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, *i. e.* $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygen; six measures mixed with three measures of oxygen diminished to five; so that it consisted of 2·6 of hydrogen, and 3·4 of a gas having the characters of nitrogen.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hemetically sealed into the tube, it occurred to me as possible that at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined: six measures of it, detonated with three measures of oxygen, diminished to rather less than one measure; a result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydrogen. That the hydrogen is in excess, may be easily referred to a slight oxidation of the platina.

---

* See Dr. Pearson's elaborate experiments, on the decomposition of water by electrical explosions. Nicholson's Journal, 4to. vol. i. page 301.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygen, hydrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxygen and hydrogen, and is never produced from those two gases alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to show that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated.* The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hydrogen gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogen dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygen gas, and the other in an apparatus in which glass, water, mercury, and wires of platina only, were present.

---

* Nicholson's Journal, August 1809, p. 258.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{12}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gases were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases for from two or three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxygen from oxymuriate of potash, was any acid or alkali exhibited in the cones; even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though, if the asbestus was touched with unwashed hands, or the smallest particle of neutro-saline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygen and hydrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely that muriatic acid in its arid state may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygen gas. I thought it possible, if water could be combined with *more oxygen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese, made red-hot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid was however generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trail in which the mixture was cooled in contact with hydrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner that the mixture could be cooled in contact with the gas produced during the operation ; and that water exhausted of air could be made to act upon the cooled mixture, and afterwards distilled from it: figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished ; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. Berthollet, that nitrogen adheres very strongly to charcoal.* The circumstances that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

---

* *Mem. d'Arcueil,* tom. ii. page 485.

The experiments of Dr. Priestly upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing there was no notable increase of the volume. At the end of the experiment, about $\frac{1}{50}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times, in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxygen, $\frac{3}{10}$ hydrogen, and $\frac{6}{10}$ nitrogen.

Mr. Kirwan observed the fact, that when nitrous gas and sulphuretted hydrogen are kept in contact for some time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited, which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results; and I found that the diminution of the volume of the gases, when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

In reasoning upon this phenomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of this lecture, contains a volume of hydrogen equal to its own. But one of hydrogen demands half its volume of oxygen to convert it into water, and nitrous gas consists of about half a part in volume of oxygen; so that, supposing the whole of the hydrogen employed in absorbing oxygen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogen over mercury, the barometer being at 29·5in. thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gases quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at 30·45in. and thermometer 52° Fahrenheit; when it equalled 2·3. The gas proved to be about $\frac{3}{4}$ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds; nitrogen and hydrogen; oxygen and sulphur combining to form one; and a part of the nitrogen and oxygen, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompound nitrogen, by agents which I conceived might act at the same time on oxygen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed: but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of Voltaic electricity.

I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant: as soon as the contact with the potassium was made, there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, of from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxygen gas.

In all trials of this kind, hydrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference that nitrogen is decompounded in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water and consequently hydrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken; the quantity did not equal $\frac{1}{8}$ of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen which disappeared in any experiment, was $\frac{1}{11}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogen, nearly equal to $\frac{1}{4}$ of the nitrogen, was produced. It cannot be said, that the nitrogen is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogen, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hydrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogen are readily decomposed by oxymuriatic acid gas, a mixture of nitrogen and oxymuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogen; but the greatest part came over densely clouded; and as nitro-muriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenor of these inquiries cannot be considered as strengthening, in any considerable degree, the suspicion which I formed of the decomposition of nitrogen, by the distillation of the olive-coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phenomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming that it has been converted into new matter. Though the iron tubes which I used were carefully cleaned; yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts; the oxygen of which in the beginning of the process of distillation, might form water with hydrogen, given off from the fusible substance; which, being condensed in the upper part of the tube, would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hydrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly one cubical inch of ammonia, four of nitrogen, and eight of hydrogen. The portion of gas given off in the second operation equalled 3·6 cubical inches; which consisted of 2·5 hydrogen, and 1·1 nitrogen. The potassium produced in the operation was sufficient to generate 3·1 cubical inches of hydrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of M. Hassenfratz,* that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which, I conceive, could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2·3 cubical inches of hydrogen were found to be generated.

In the experiments detailed in page 53 of the last volume of the Transactions,† a loss of nitrogen, and a production of hydrogen, was perceived in a case in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible that ammonia might have been regenerated by hydrogen from the naphtha, and absorbed by that fluid; and a part of the hydrogen might likewise proceed from the decomposition of the naphtha: and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

---

* *Journal des Mines, Avril* 1808, p. 275.

† Phil. Mag. vol. xxxiii, p. 8.

Even the considerable excess of hydrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451,* it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me, against the mode of explaining the phenomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conductad according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygen was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phenomenon.

The experiment of producing an amalgam from ammonia which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygen in this substance; at the same time it led to the suspicion, that of the two gases separated by electricity, one, or perhaps both, might contain metallic matter united to oxygen: and the results of the distillation of the fusible substance, from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

---

* Phil. Mag. vol. xxxiv, page 339.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an apparatus (of which a figure is attached to this paper) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour, which if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gases,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogen and nitrogen, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive-coloured matter formed on the metal; but though in this case nearly eight cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to—15°, there was a very feeble indication given of the addition of hydrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hydrogen was to the nitrogen in the average proportions of from 73·74 to 27·26; the proper corrections being made, and the precautions before referred to being taken.*

Assuming the common estimations of the specific gravity of ammonia, of hydrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807, would be supported by these new experiments; but as the moisture and oxygen visibly separated cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of the ammonia, I resolved to investigate, more precisely than I had reason to think had been hitherto done, the specific gravities of the gases concerned in their dry state; and the very delicate balance belonging to the Royal institution placed the means of doing this in my power.

Nitrogen, hydrogen, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at 30·5$^{\text{in}}$. barometer, 51° Fahrenheit's thermometer.

---

* Philosophical Transactions, 1809, page 459. M. Bertholett, jun. in the second volume of the Memoirs of Arcueil, has given a paper on the decomposition of ammonia, and he enters into an examination of my idea of the oxygen separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent. and at the same time he confutes some experiments which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia. His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxygen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chymist on the direct decomposition of ammonia seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver, which I conceive has occasioned him to overrate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible that the volume may be exactly doubled, and that the nitrogen may be to the hydrogen as one to three; but neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

For nitrogen, the 100 cubical inches............29·8 grains.
For hydrogen, ditto..2·27
For ammonia...18·4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gases evolved, there is a loss of $\frac{1}{18}$;* and if the smallest proportion be taken, the loss will be nearly $\frac{1}{12}$.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is *possible*, that the minute quantity of oxygen which appears to be separated is not accidental, but a result of the decomposition; and if hydrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogen and hydrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry;

---

* 100 of ammonia at the rate of 185, will give 136·9 of hydrogen, weighing 3·1, and 48·1 of nitrogen, weighing 14·33 grains; but 18·4—17·4=1, and at the rate of 180, 133 of hydrogen, weighing 3·01 and 47 of nitrogen weighing 14, and 18.4—17=1·4.

and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct than when pure mercury was used.

By exposing the mercury to a cold of —20° Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and, when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{5}{10}$ to $\frac{6}{10}$ of hydrogen produced.

As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide) must contain about 48 per cent. of oxygen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases.*

If hydrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxygen, and 34 of basis; but if the opinion be adopted, that hydrogen and nitrogen are both oxides of the same metal, then the quantity of oxygen in nitrogen must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis, of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hydrogen, and metallic, by combining with a greater proportion.

---

* Even in common air, the amalgam evolves hydrogen and ammonia, nearly in these proportions, and in one experiment which I lately tried, there seemed to be no absorption of oxygen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rapidly absorb oxygen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygen had been absorbed; but this difference might have arisen, partly at least, from the mixture of hydrogen. Whether in any case the amalgam absorbs oxygen gas, is a question for further investigation.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at $\frac{1}{12000}$; but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56 [page 135 preceding] be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{16000}$ of new matter, on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully, these ideas, and I shall conclude this section by stating, that though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

IV. *On the Metals of Earths.*

I have tried a number of experiments with the hopes of gaining the same distinct evidences of the decomposition of the common earths, as those afforded by the electro-chemical processes on the alkalies, and the alkaline earths.

I find that when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine or glucine, slightly moistened and placed in hydrogen gas; the iron becomes brittle and whiter, and affords by solution in acids, an earth of the same kind as that which has been employed in the experiment.

I have passed potassium in vapor through each of these earths, heated to whiteness in a platina tube : the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali, formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour, so that there is great reason to believe, that the silex had been either entirely or partially deoxygenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silex. The part which did not inflame, was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

---

* The results of this experiment are opposed to the idea that potassium is a compound of hydrogen and potash or its basis; for if so, it might be expected that the hydrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination. I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogen as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapor of the water, which must have been constantly supplied.

Potassium in acting upon alumine and glucine, produced more hydrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable that even after ignition, water adheres to these earths.

The results of the action of the potassium were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks,* and leaving behind alkali and earth, and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them; which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury, whilst the tube was hot; but the alkaline metal gave the characters to the amalgam, and though in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded, by solution in acids, evaporation, and treatment with reagents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

---

* The pyrophorus from alum, which I have supposed in the last Bakerian lecture to be a compound of potassium, sulphur and charcoal, probably contains this substance likewise.

Though I could not procure decided evidences of the production of an amalgam from the metals of the common earths, yet I succeeded perfectly by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder, and which when acted upon by weak muriatic acid, gave off hydrogen gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygen which they absorb; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

On an hypothesis which I have before brought before the society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter, and that oxygen and inflammable bodies are in relations of attraction which correspond to the function of being negative and positive respectively, it would follow that the attractions of acids for salifiable bases would be inversely as the quantity of oxygen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808, concluded that barytes must contain least oxygen of all the earths, and that the order as to the quantity of inflammable matter must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxygen of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90·5* of metal per cent. strontites 86.† lime 73·5,* magnesia 66.‡

The same proportion would follow from an application of Mr. Dalton's ingenious supposition,§ that the proportion of oxygen is the same in all protoxides, and that the quantity of acid is the same in all neutral salts, *i. e.* that every neutral salt is composed of one particle of metal, one of oxygen, and one of acid.

---

* Mr. James Thomson, Nicholson's Journal, 1809, p. 175, and Berthier.

† Mr. Clayfield. Thomson's Chemistry, vol. ii. p. 626, 629.

‡ Murray's Chemistry, vol. iii. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis being inversely as the quantity of oxygen contained by the basis, though gained from the comparison of the electrical relations of the earths, with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which, in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. Gay Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygen, might (as far as it is correct) be inferred from Mr. Dalton's law, though this ingenious chemist states that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxygen, the same in all protoxides, and there is a proportion of acid, the same in all neutral salts; and new proportions of oxygen and of acid are always multiples of these proportions. So that if a protoxide in becoming a deutoxide takes up more acid, it will be at least double the quantity, and in these cases the oxygen will be strictly as the acid. Mr. Dalton's law even provides for cases to which M. Gay Lussac's will not apply, a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxysulphat of iron perfectly formed, (as some experiments which I have lately made seem to show,) there is probably only a single

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition* of phospate of alumine, alumine would appear to contain about 56 per cent. of metallic matter.

M. Berzelius,† in a letter which I received from him a few months ago, states, that in making an analysis of cast iron, he found that it contained the metal of silex, and that this metal in being oxidated took up nearly half its weight of oxygen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxygen, which agrees very nearly with the quantity of hydrogen and ammonia produced from the amalgam.

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of combining with phlogiston, and though Lavoisier insisted upon this analogy with his usual sagacity, yet still the alkalies, earths, and oxides have been generally considered as separate natural orders. The earths, it has been said, are not

---

proportion of acid; and in the super-tartrite of potash there is only a single quantity of oxygen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

* Thompson's Chemistry, vol. ii. p. 581.

† In the same communication this able chymist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

‡ I take the proportions of the volumes from the very curious paper of M. Gay Lussac, on the combinations of gaseous bodies, *Mem. d'Arcueil*, tom. ii. page 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by M. M. Gay Lussac and Thenard.

precipitated by the triple prussiates, or by the solutions of galls;* and the alkalies and alkaline earths are both distinguished by their solubility in water; but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalies and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and M. Guyton) precipitate the triple prussiate of potash; the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutro-saline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalies and earths with oils, to form soaps; and of the earthy soaps, some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxygen, approach very near in their general characters to zircon, silex, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals!

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

---

* *Klaproth, Annales de Chimie*, tome x. p. 277.

Cast iron from a particular spot will make only cold-short iron; whilst, from another spot, it will make hot-short; but, by a combination of the two in due proportion, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, M. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

Hydrogen is the body which combines with the largest proportion of oxygen, and yet it forms with it a neutral compound. This, on the hypothesis of electrical energy, would show that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxygen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phenomena presented by sulphuretted hydrogen are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shown, and as is further proved by the researches of M. M. Gay Lussac and Thenard, is a compound of a body unknown in a separate state and water. The water, I believe, cannot be decompounded, unless a new combination is formed: thus it is not changed by charcoal ignited in the gas by Voltaic electricity; but it is decompounded by all the metals; and in these cases hydrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygen being found in the new compound. This, at first view, might be supposed in favour of the idea that hydrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitro-muriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxygenation.

That nitrogen is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches that hydrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus,* and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter, to acid matter, might be supposed capable of being ascertained by their relations to oxygen, or by the peculiar state of their electrical energy.

---

* The electrization of sulphur and phosphorus goes far to prove that they contain combined hydrogen. From the phenomena of the action of potassium upon them in my first experiments, I conceived that they contained oxygen, though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxygen, till I discovered that similar phenomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hydrogen, and on phosphuretted hydrogen, I find that the phenomena differ very much according to the circumstances of the experiment, and in some instances I have obtained a larger volume of gas from potassium after it had been exposed to the action of certain of these bodies, than it would have given alone.

The whole tenor of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phenomena with at least equal facility.

If hydrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxygen, hydrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases, with hydrogen; the earths, the fixed alkalies, metallic oxides, and the common acids, will be compounds of the same bases, with water.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are; First, the properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hydrogen or oxygen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hydrogen, whether in its simple form, or in combination with one proportion of oxygen, or a double proportion, from my experiments seems to combine with the same quantity of

These experiments are still in progress, and I shall soon lay an account of them before the society. The idea of the existence of oxygen in sulphur and phosphorus is, however, still supported by various analogies. Their being nonconductors of electricity is one argument in favour of this. Potassium and sodium, I find, when heated in hydrogen, mixed with a small quantity of atmospheric air, absorb both oxygen and hydrogen, and become nonconducting inflammable bodies analogous to resinous ond oily substances.

alkali. Tellurium, whether in the state of oxide or of hydruret, seems to have the same tendency of combination with alkali ; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

Second, the facility with which metallic substances are revived, in cases in which hydrogen is present. I placed two platina wires, positively and negatively electrified from 500 double plates of six inches, in fused litharge ; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced ; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed : the difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hydrogen is essential to the production of the metal.

Third, oxygen and hydrogen are bodies that in all cases seem to neutralize each other, and therefore in the products of combustion it might be expected that the natural energies of the bases would be most distinctly displayed, which is the case; and in oxymuriatic acid, the acid energy seems to be blunted by oxygen, and is restored by the addition of hydrogen.

In the action of potassium and sodium upon ammonia, though the quantity of hydrogen evolved in my experiments is not exactly the same as that produced by their action upon water; yet it is probable that this is caused by the imperfection of the process ;* and supposing potassium and sodium to produce the same quantity of hydrogen from ammonia and water, the circumstance, at first view, may be conceived favourable to the notion that they contain hydrogen, which under common circumstances of combination will be repellant to matter of the same kind: but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then whenever bodies known to contain hydrogen are decomposed by a metal, the quantities of hydrogen ought to be the same, or multiples of each other. Thus, in the decomposition of ammonia by potassium and sodium, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is given off; and in the action of water on potassium to form potash, the same quantity of hydrogen ought to be expelled. From my analysis* of sulphuretted hydrogen, it would appear, that if potassium in forming a combination with this substance sets free hydrogen, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogen, will liberate the same proportion of hydrogen as during its solution in diluted sulphuric acid, and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hydrogen, because, in acting upon different combinations containing hydrogen, they produce the evolution of equal proportions of this gas, then it might be proved that almost any kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

---

* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hydrogen evolved ; *i. e.* whenever the metals of the alkalies act upon ammonia, supposing this body to be composed of three of hydrogen, and one of nitrogen, in volume, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalies and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

---

* The composition may be deduced from the experiments in the last Bakerian lecture, which show that it contains a volume of hydrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hydrogen, and 32·73 of sulphur. When sulphuretted hydrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hydrogen, it was found that when these gases were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common state; the phosphorus was dark coloured, and the arsenic appeared as a brown powder, both were probably hydrurets: this is confirmed likewise by the action of potassium upon arseniuretted and phosphuretted hydrogen; when the metal is in smaller quantity than is sufficient to decompose the whole of the gases, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogen contain in equal volumes, more hydrogen than sulphuretted hydrogen, probably half as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogen, it would appear that 100 cubic inches of the first, weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

Potassium, sodium, iron, mercury, and all metals that I have experimented upon, in acting upon muriatic acid gas, evolve the same quantity of hydrogen, and all form dry muriates: so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hydrogen in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygen, which may be supposed to exist in hydrogen, that might be applied to explain the composition of nitrogen from the same bases, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenor of our chemical reasonings.

I shall not at present occupy the time of the society by entering any further into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined to enable us to decide upon their nature, and their relations, or to form any general theory concerning them which is likely to be permanent.

Explanation of the Figures.

Fig. 1. The apparatus for electrizing potassium in gases. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water, out of the contact of air, page 148. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a*, *d*, *c*, the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube of water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of 1000*l.* has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has has not been put into action; but reasoning from the effects of that part of it which has been used, some important phenomena may be expected from so great an accumulation of electrical power.

LEHMAN,

ON THE CHEMICAL PHENOMENA OF RESPIRATION.

Philadeldhia, *June* 26, 1811.

DEAR SIR,

IF you think the following observations possess any merit, you will oblige me by inserting them in the Baltimore Medical and Philosophical Lycæum.

Before that illustrious chemist, Priestley, discovered oxygen gas, physiologists were involved in darkness, respecting the functions of respiration. When the component parts of the atmosphere were made known, this function was closely examined. Theory, gave way to theory; and for some time there was nothing remaining but a mass of ingenious rubbish. One fact led the way to another; and in this manner, nearly every one who wrote on it, added something to its elucidation. There are few subjects that have excited more attention among philosophers, than the one under consideration. It is certainly important. The wheels of the animal machine are kept in motion by the agency of respiration. In the following observations, I shall consider the changes that take place in the blood and atmosphere during respiration; and in endeavouring to account for these phenomena, I shall also observe some of the uses of respiration. It would be an unnecessary and useless trouble, to review the different theories that have attracted the notice of the literary world.

Lavoisier's was simple and beautiful, but fell at the touch of investigation. Crawford's dwindled into obscurity before the rays of truth. Perhaps I should not have attempted to write on a subject which has been so often investigated, had not some remarks of Mr. Mitchell, in a late number of Dr. Coxe's Medical Museum, engaged me. In the first place, it is known to every physiologist, that the blood in the right side of the heart, is of a purple hue; but after circulation through the lungs is carried to the left side, of a scarlet colour. This being assumed, it remained for philosophers to explain, in what manner the change was produced. Many hyphotheses and conjectures were offered, but the generality of them were overturned by reason, and soon forgotten. The gentleman alluded to above, says, that the vermilion colour of the blood, is the consequence of the emission of carbon: but his proofs are not sufficient to support this position. If blood in any manner, be deprived of its carbon, and assume a florid appearance, is it not rational to suppose, agreeably to Mr. Mitchell, that it will not become dark unless again impregnated with carbon? This is so plain, that no one will pretend to dispute it. Will arterial blood assume a dark hue by exposure to the atmosphere? Yes. Does not this fact, then, itself, invalidate the above idea? In what manner can Mr. Mitchell reconcile it with his opinions? It is a barrier which must be removed to admit a free passage for his theory; but more facts appear to prove it erroneous. After the tourniquet has been applied; on the division of an artery, the blood will have the venous appearance. Now, certainly, according to Mr. Mitchell, it should be of a scarlet hue; for he says, it loses its carbon in the lungs, and this colour is the effect of its deprivation. If he were correct, the blood should be receiving a supply of carbon, which is not the case: because, all communication is cut off. Place florid blood in such a situation, that it can be affected neither by carbon nor oxygen, and it will take on a dark appearance: but expose it to oxygen, and it becomes again of a vermilion hue. This clearly shows that carbon is not the cause of its dark colour. The change is more correctly, though relatively explained by the want of oxygen. The Azotic, hydrogenous and all the gases that do not contain oxygen, alter the scarlet blood to purple. How can this phenomenon occur according to Mr. Mitchell's theory? It would be absurd to say carbon had this effect. What solution, then, can be given? The oxygen must be attracted by these gases, and as the colour of the blood is altogether dependent on the presence or absence of oxygen, it assumes a purple dye. Speaking of Dr. Priestley's changing the colour of blood in a bladder, by oxygen, he observes, " If this gas could penetrate the compact texture of a bladder, and act on a fluid beneath it, why could it not also pass through the blood in a bowl and affect every part of it? I will not say whether this should be expected or not. However, I am pretty well satisfied, that the scarlet dye is not the effect of oxygen uniting with the carbon of the blood, and forming carbonic acid gas. " This, says he, " will also explain the fact, that after the surface of blood drawn out into a vessel has been rendered florid by oxygen, if the latter be removed the blood will resume its dark appearance."

The explanation of this occurrence is somewhat difficult. Does Mr. Mitchell mean the oxygen which has united with the carbon, or the oxygen of the atmosphere? If the former, it may be so; if the latter it will not; for, consistent with his theory, the carbonic acid gas already made, would sink lower, or at least being heavier than the other airs, would maintain its place: therefore it does not account for the fact. The common illustration is, that the oxygen in the blood unites more intimately with the carbon. The atmosphere, is composed, agreeably to Thompson, of about one fifth oxygen, to four fifths nitrogen. After inspiration, it is materially altered; nearly all its vital air is absorbed, and Mr. Davy says a small portion of nitrogen with it.

The question has been asked, what becomes of the oxygen? Is it discharged in the same mode it entered? There is no necessity for this, and I think it *unrensonable* to suppose it. Besides, a portion which unites with the carbon conveyed into the blood-vessels by the lymphatics forming an oxyde of carbon, a quantity combines with the animal fibre, and is the principle from which excitability is derived. How this combination takes place, time must determine. *Animus providet futura*, is a just saying, and many of the scientific world expect that the genius and penetration of some philosopher will soon bring to light this hidden secret of nature. From what I have stated it appears, 1st. That the bright hue of the blood, is the effect of the absorbtion of oxygen. and the deficiency of this gas reasoning a priori, would be the want of this colour.

2d. To the strong attraction existing between the blood and oxygen is to be attributed the decomposition of the atmosphere. Part of the oxygen joining the oxyde of carbon in the lungs, expounds the formation of the carbonic acid gas; and the loss of a small proportion of nitrogen carried into the circulation is owing to the affinity between that gas and vital air. These facts are of the first magnitude. The wonders of respiration are yet open to inquiry. Who knows but La Grange's theory ere long may meet the fate of its predecessors? So fluctuating is the science of chemistry, that we can scarcely affirm any thing is, or is not correct. Every new opinion should be examined, and error be detected in its infancy. I shall now make a few remarks on my last proposition, viz. The uses of respiration. When we see that every animal in nature, has respiratory organs we may with propriety conclude, that this function is indispensably necessary to life.

1. It is impossible for blood not possessing the principle of vitality to sustain the human body. Respiration performs the noble office of affording this principle which is oxygen.

2. It was for some time thought that the union of oxygen with the blood was the only source of animal heat; this Dr. Rush has long since shewn, to be erroneous. It generates

heat, so does almost every operation in the animal economy.

3. It gives a scarlet dye to the blood and without oxygen life would soon cease.

4. It conveys out of the system, gases which are known to be deleterious.—

5. The air inhaled increases the irritability, of the muscles.

6. It assists in forming the human voice, by which man is so much exalted above the brute creation. Many other uses of respiration might be mentioned, but these are sufficient to shew the connexion between it and life.

With respect I am yours

GEORGE F. LEHMAN.

……………

Hall on the Influenza of 1807.

Baltimore, 2d July, 1811.

SIR,

IT is deemed unnecessary to offer an apology for the history of an epidemic, however deficient it may be in arrangement, provided it can add by a single fact to the general stock of medical knowledge.

Enclosed you will receive a short account of the influenza of 1807, as it appeared in the upper section of Harford county, (Maryland,) about N. latitude 39, 40, and 1 E. longitude. Provided you think proper you are at liberty to insert it in the Lycæum.

The influenza made its appearance in the abovementioned section of country about the last of August, and advanced with a rapidity unequalled by any other epidemic. The temperature of the weather was rather more cool and dry, than usually occurs at this season. The thermometer generally ranged between 70 and 83, of Fahrenheit, seldom exceeding 85.—

The disease passed by children (with a few exceptions) and attacked those of both sexes with equal violence who were arriving at, or had attained the age of puberty. Persons advanced in life who had been debilitated by disease, or any other cause, suffered extremely by its visitation.

The *premonitory* symptoms were violent and long continued sneezing, (indicating an irrtaition of the Schneiderian membrane), hoarseness, slight cough and titillation about the superior portion of the trachea; and an obtuse pain of the head and muscles of the neck, back, and inferior extremities. An accute pain over the frontal sinuses, or glassy appearance of the eyes often indicated the approach of this unwelcome visitor.—

The above symptoms were succeeded by a rigor or chill of different degrees of violence, followed by a fever of the inflammatory grade. A perspiration sometimes supervened and often afforded relief.

In some persons all the above symptoms shewed themselves; in others a few only occurred.

In a generality of cases, the cough was the most distressing symptom; and in aged persons, occasionally equalled the pertussis in violence and duration.

By many, acute pains were felt darting through the thorax, more particularly during the paroxysm of coughing; and in several instances which came under my care severe pleurisies ensued.

It produced pneumonia notha in a few persons who had previously laboured under pulmonary desease, and suddenly proved fatal.

A tumefaction of the amygdalæ was not an uncommon symptom, and it subsided with the hoareness which was generally removed by expectoration.

The fever evidently partook of the intermittent type and had its exacerbations generally in the evening. The pulse was often full and tense, more generally moderately so; sometimes natural, but seldom below the usual standard; except when depressed as in pneumonia notha.

The tongue exhibited (with few exceptions) more or less, of the white or febrile appearance, according to the severity of the disease.

The bowels were generally costive; in a few persons of the intestinal temperament a diarrhœa took place.

The fever seldom continued more than four or five days, unless in those cases where the lungs were much affected.

The disease began to decline in four or five weeks from its first appearance, but left many afflicted with a troublesome catarrh.

The mode of treatment, in this epidemic varied according to the symptoms; when the pulse was full and tense, the lungs oppressed, breathing difficult, and acute pains were felt in the thorax, venæsection was resorted to with immediate advantage; and in those cases which terminated in pleurisy the lancet was freely used; often twice a day. Here also other auxiliaries were thrown in, as emetics, vesicatories, and such remedies as are generally termed antiphlogistic. The loss of blood moderated the fever, relieved the pains of the thorax, head and extremities, and palliated the cough. It was repeated according to the pulse and the violence of the symptoms, and in no case proved injurious. Many persons prescribed it in their families without consulting a physician; even when the severity of the symptoms did not appear to require it. As far as my information extended no ill consequences were the result. I mention this more particularly, because in the influenza of 1790, Dr. Rush remarks that in several cases blood-letting produced alarming results.

Demulcent drinks were beneficial throughout every stage of the disease, by removing the disposition to frequent coughing which almost uniformly attended.

But no article of the materia medica in my hands afforded such speedy and obvious relief to this distressing symptom as * digitalis which was generally exhibited in the form of

---

* During the prevalence of this epidemic in Baltimore, it generally happened that the cough outlived all the other symptoms.

tincture. It acted as by incantation in relieving the cough, and when taken at night procured undisturbed repose. The tinctura opii was occasionally added to the digitalis, but the latter acted as an anodyne, when the former failed. From the extensive manner in which I administered the digitalis, and the great relief which it uniformly afforded, I am induced to consider it as a most valuable article in the treatment of the influenza.

Long after the subsidence of general fever a chronic inflammation of the lungs and their appendages, the bronchiæ and trachea required the physicians attention. It often resisted blisters, anodynes, emetics, diaphoreties, and all the demulcents usually resorted to in pulmonary affections; but the tincture of digitalis succeeded radically in every instance, except one, which terminated in phthysis pulmonalis from the neglect of bloodletting.

Several writers have derogated from the character of the lancet in this epidemic, or in others that have received the same appellation. Whatever grade of inflammation may have constituted this disease in other places or at other times, the following facts are vehement presumptions, that the most prominent error in its treatment was the parsimonious use of this noble antiphlogistic.

1st. Notwithstanding the disease, when left to nature or a temporizing treatment always observed one uniform, protracted routine; bloodletting employed early and copiously always abridged its duration, and often obviated the vexation of a tedious expectoration.

2d. It often prevented the cough, and rendered a secondary treatment with the digitalis unnecessary.

3d It prevented that slow convalescence so frequently the consequence of reiterated pulmonary irritation.

4th. In several instances a spontaneous hemorrhage from the nose, far more copious then the artificial hemorrhage from the arm, put a period to the disease in a time much shorter than that required by the ordinary use of the lancet. On the 13th of August, Daniel Steuart an athletic man aged thirty eight, was attacked with the prevailing epidemic. About twenty eight hours from the accession of the chill, I was called to arrest a hemorrhage from his nose. He had discharged about four pounds of blood

Purgatives were seldom administered with advantage. Gentle laxatives only were used to obviate constipation.

Hard labour, or violent and long continued exertion removed the disease in a few cases : In my own, severe exercise on horse back effected a cure.

and still the pulses were tense, and the symptoms only mitigated. I suffered the blood to flow till nearly seven pounds had been avacuated, when a compress was applied to the bleeding vessel and the hemorrhage ceased. Although this was one of the most formidable attacks that occurred to me, it was the soonest cured. In forty-two hours from its commencement it was completely quelled, and the convalescence was quick and uninterrupted, except from the voracious appetite of the patient, which required to be restrained. This case is stated merely as an illustration of the principle; not because it is solitary. There were several very delicate subjects, who recovered rapidly after hemorrhagies from the nose, far more abundant than the most sanguinary bleeder would have been willing to draw from the arm, n a disease whose aspect was so little imposing as not to assume the most prominent characteristics of inflammation.

There was one more fact connected with this subject, that may one day reflect light on the remote cause of the influenza or the pathology of asthma. At the commencement of the epidemic, several persons who were suffering under an asthmatic paroxysm were suddenly relieved: their respiration became natural and easy, while the lungs seemed to have resumed their healthy functions. The influenza began to decline in about three weeks from its first appearance ; and during this time no asthmatic person was visited by a paroxysm. Nor did any of them experience the slightest symptom of the epidemic, which pervaded all other ages, sexes and conditions of adults. This jubilee to afflicted humanity was however but fleeting, for no sooner had the monarch of the day deposed his authority, then the asthma was reverberated with more than usual violence. Four persons who were under my care testified to these facts, but as it was still possible that accidental coincidence might have accounted for them, I resorted to other testimony and found that eight more asthmatics concurred with my own patients.

Ed.

Many hundreds who were attacked by it, continued to pursue their usual occupations; others who kept their houses were not confined to bed. In many it proved so mild as scarcely to demand attention.

The appetite often continued unimpaired, particularly when the disease was not violent in its symptoms.

It is a fact confirmed by the observation of the inhabitants, that bilious remittent and intermittent fevers which are endemial in this district of country, (from its contiguity to the river Susquehanna, and a considerable creek), were never so rare as during the continuance of the influenza. The autumn after its prevalence was unusually exempted from the autumnal forms of fever.

It has been the opinion of some physicians of eminence, that the influenza is a contagious disease, but the belief that it is only propagated by contagion is invalidated by the fact, that several years would be required, to spread it in this manner as extensively, as observation teaches us, it has been propagated in a few weeks. That it does not depend altogether on the qualities of the atmosphere, is evident from its appearance at all the various seasons of the year, and by the fact that it extends its ungracious influence with equal, and almost electrical rapidity, over countries which are congealed by the cold of a northern winter, and those which are schorched by the ardent rays of a vertical sun. May we not venture an opinion, that a comparison of the histories of this disease which have already been given to the world, with those which posterity may produce, will hereafter afford a clue, by which a knowledge of its phenomena may be obtained?

RICHARD W. HALL.

WILLIAMSON

On the connection between Scrofula and phthysis Pulmonalis

Baltimore, May 30th, 1811.

IN my last I concluded my remarks directly relative to scrofula. I shall now proceed to give my sentiments on this disease in conjunction with phthysis pulmonalis.

April 4th, 1808.

I am now attending a patient in the last stage of phthysis pulmonalis, whom I first saw about six months ago, and who has strumous tumors of considerable size on the neck, evidently of a scrofulous nature. These tumors are of several years duration.

This fact has, I conceive, a tendency to prove that there is some truth in the old opinion of a relation between the scrofula and phthysis pulmonalis. Dr. Mead has observed, "*that those persons are most liable to ulcers in the lungs, who have strumous swellings in childhood, or youth ;* and he adds; "that most experienced physician Ratcliff, used to say, that most consumptions in England and in other cold countries, were of the strumous kind." We often see in dissecting the bodies of patients, who have died of consumption, the lungs filled with tumors or indurated glands, which had suppurated and discharged matter. Although I believe in the probability of the phthysis pulmonalis being frequently, and perhaps in all hereditary cases, ingrafted on scrofulous habits ; yet, I am very far from believing that persons of this class or habit, are the only ones subject to that disease.

When separate, these diseases are either of them formidable in their nature, and frequently desperate in their event : when combined they must be doubly so !

If there be different species of the phthysis pulmonalis, it is rational to suppose that different methods of treatment should be adopted ; and in order to accomplish the desired effect, it is the incumbent duty of every practitioner of medicine, to endeavour to distinguish between different forms or species of the same disease. For, to endeavour to cure the phthysis of a scrofulous nature, by remedies which are proper in that of an inflammatory or pneumonic order, would be as irrational, as it would be to attempt to extinguish a fire by adding fuel. Although the scrofula has generally been considered an incurable disease; yet, fortunate experience has taught us that it may sometimes be cured. If it be possible to cure it when affecting the glands perceptible to the eye, may we not flatter ourselves, that it is also curable when seated in these glands situated in the interior of the system?

Facts are incontrovertible, and they have proved that the phthysis is sometimes of an inflammatory nature, and only to be remedied by the antiphlogistic treatment. But facts have never proved the antiphlogistic treatment salutary in scrofula. On the contrary, they have proved it exceedingly pernicious. If therefore the consumption is ever of a scrofulous nature, and facts have proved the antiphlogistic treatment improper in scrofula, analogy should teach us the impropriety of adopting this mode of treatment in diseases of that nature. Supposing then that there are these two different forms of phthysis, and granting that our opinion respecting the different method of treatment be correct, we can very easily perceive, that by mistaking or confounding them, incalculable injuries may arise; and that all the anxiety which a sympathetic mind can possess, and all the ardent wishes that humanity can excite, with our greatest exertions and strictest attention, must prove abortive. When, on the contrary were we able to distinguish completely between the different forms of disease, and prescribe according to their nature, we might, perhaps not unfrequently have our solicitude crowned with a fortunate issue.

It was formerly believed that the phthysis pulmonalis was an incurable disease; and whatever physicians of great eminence of the present day, may have asserted to the contrary, it is yet doubtful with me, whether a confirmed phthysis can ever be cured.

The situation of the lungs, with their important and active functions render it very difficult to cure them when diseased, especially if the disease be of an extensive or chronic nature. It is in consequence of this, with that of confounding or mistaking the different species, and consequently treating it improperly, that the consumption remains, as it ever has been, almost always incurable. But in this enlightened age, when the rays of phylosophy are radiating from their centre, and unveiling to the industrious student truths before unknown; (many of which have been discovered in chemistry, and not a few in the science of medicine) may we not rationally hope, that she will be still more bountiful, and continue to guide us by her divine light into the mysterious paths of our profession, and illuminate them so as to enable us to discriminate more completely between diseases of a different nature or different species of the same disease, and teach us by her laws how to prescribe for them, when known. Although we dare hope for an event of this invaluable nature; yet we dare not anticipate that day, when the consumption will not be so formidable a disease as to frequently terminate fatally.

> "Though skilfully, the sage perform his part,
> It will oft tryumph o'er the healing art."

The grand improvements or discoveries which I anticipate relative to the phthysis pulmonalis, are more of a preventive than a curative nature.

As I do not intend to dilate on the consumption in general, the subsequent remarks will be principally confined to that of a scrofulous nature.

If the scrofula is difficult to cure, when the glands of the neck, &c. are the seat of disease, it must be much more so, when seated in the lungs. For independent of the difficulties attending the treatment of this disease in general, there are additional ones, when the lungs are the seat of it.

The bath, as mentioned in my former essay, I consider very beneficial in scrofula, when the lungs are not the seat of disease, but when they are, it is always a doubtful remedy, if not a pernicious practice. I have tried it in the cholera infantum, some months after the tussis convulsiva, and when this last disease had apparently entirely disappeared; but it excited a morbid action in the lungs, and we were compelled to discontinue it it as pernicious. Other cases might be adduced to prove its injurious effects in most diseases of the lungs attented with cough, or where there is a great predisposition to affections of this nature, but this is a fact too well known to need dilating on by me.

It is possible the tepid bath might be used with advantage in scrofulous affections of the lungs, as the cutis generally sympathises with the diseased organ. While some parts of it are cold, others are preternaturally warm; it is however most generally parched and dry. The cold bath, by carrying off the heat too rapidly, causes a contraction of the skin and increases the flow of blood to the heart and lungs; whereas the tepid bath, if the skin be cold, would produce a determination to the superficies, and if parched would tend to equalize the animal heat, and produce a salutary perspiration. However great care is requisite in using this bath, if it be too warm, it will increase the general excitement, and may be productive of much injury. I am induced to believe that the air bath with friction, properly conducted, would be highly salutary in scrofulous affections of the lungs.

The use of external or local remedies, I think very salutary in ordinary cases of scrofula; they can however be had access to but very imperfectly, if at all, when the lungs are the seat of the disease. Those medicines inhaled should, I suppose, be considered of the local order.

Dr. Pearson of Birmingham, has spoken in exalted terms of the good effects of ether and cicuta, when inhaled, in the phthysis pulmonalis. If this compound is ever salutary, I suspect, it must be in the scrofulous species of this disease. The Doctor adds one scruple or half a drachm of the powdered leaves to every ounce of sulphuric ether. After the ether becomes sufficiently impregnated, one or two tea spoonsfull are put in a wine glass or cup, then the patient holds it to his mouth and inspires the vapour that arises from it, until the ether is evaporated. This is repeated three or four times a day.

The first effects of this application, says the doctor, "are, an agreeable sensation of coolness in the chest, an abatement of the dyspnæa and cough, and after ten minutes or a quarter of an hour, easier expectoration." The doctor then proceeds to say, what the ultimate effects are, but here he has recourse to conjecture. Other proper measures are not to be neglected as this is not to supersede the use of other medicines. After mentioning what the ultimate effects are, he proceeds: "to assert that all these beneficial consequences will flow from its application in every species and degree of the phthysis pulmonalis would be adopting the language of quacks, and insulting the understanding of every one experienced in the profession."

From this, and the following sentence, the doctor must have been at least, in part aware of its being limited in its effects. I shall only remark, says he, "that it appears to be best suited to the florid, or what is termed the scrofulous consumption." He tried the ether with several volatile substances, but the cicuta was the most efficacious. "The narcotic particles of the cicuta, conveyed in this manner, along with the ether, to the diseased lungs, act as a topical application with the best effect."

The cicuta we know has long been celebrated in scrofula. The ether, no doubt increases the exhalation from the diseased parts, and carries off the superabundant heat, the sensation experienced warrants this conclusion. This remedy may also operate as a local stimulus and excite the absorbents to an increased action.

The incipient or first stage of the phthysis pulmonalis is that, in which this medicine must have the most benign effect. Dr. Pearson, it is probable, never used it except in confirmed cases; and experience taught him it was inefficacious in the last stage of the disease, or in complicated affections, as he thus observes. "Where the pulmonic affection is complicated with mesenteric obstructions or diseases of the other viscera, or a dropsical condition, it affords but transitory relief; and in the very last stage of the disease the proper time of using it is past."

But the two most forcible causes why this disease is more fatal when the lungs are the seat of it, than when the external glands are; are first, the peculiar and active situation of the diseased organs. Second, the difficulty, or almost impossibility of timely discovering the disease. I have no doubt, but that the lungs may be in a morbid state a long time, without being suspected of disease.

The first of these reasons is so strikingly obvious that it were useless to dwell on it, further than to observe the necessity of studiously attending to the various diseases to which the lungs are subject, to be always careful to distinguish them, and to prescribe for the symptoms of disease and not for its nosological name.

In diseases of the lungs, all the minutiæ should be strictly attended to, not only respecting its nature and treatment, but also relative to the patients constitution, habits, family, present and former place of residence, diet, clothing, occupation, and every thing that can have any influence on the disease or mode of treatment.

Second, the difficulty or almost impossibility of timely discovering the disease. Fernelius justly remarks, when speaking of a vomica of the lungs. "This is a very obscure and concealed disease, and often not apparent either to the patient or physician; so that the patient neither alters his course of living, nor thinks himself ill, but bears about unknowingly in his breast the seeds of death." That this is the case in scrofulous affections must be obvious to every person acquainted with the nature of the lungs, as well as of this disease. Therefore, when applied to by a patient who has a morbid affection of this viscus, we should be particularly careful to ascertain the nature of the disease ; and if the symptoms, with the general constitution of the patient indicate a disease of this nature, we should treat it as such. The treatment proper for scrofula in general will be proper in this case, with a few exceptions, and these a judicious practioner will be able to detect. But the treatment proper for a pneumonic phthysis would be very pernicious in this species of consumption.

Bloodletting, mercury, &c. which have been proved by our venerable countryman, Rush, to be salutary in many cases of phthysis, must be injurious in that of a scrofulous nature, tending to debilitate the general system, and all debilitating remedies are improper in scrofula. When this disease is evidently about to terminate in the phthysis, if there be a sense of engorgement or fulness in the lungs, a few ounces of blood may be drawn, but let this be done with judgment and not without sufficient indication.

If the preparations of mercury are ever beneficial in this species of disease, it is only when given in small quantities, and combined with tonics. For, if it is continued until it has a general effect on the system, it debilitates, and, I believe, tends to produce suppuration in the scrofulous tumors ; and when these tumors suppurate, they are much more difficultly, cured. Hence the necessity of distinguishing between scrofulous and other schirrous tumors, for while mercurials are very pernicious in the former, they are highly salutary in the latter. I know of no better way of distinguishing these diseases, than by consulting the constitution of the patient.

In addition to the indication of the patient's constitution, there are distinguishing symptoms between the scrofula and other tumors, so that no physician who has had much practice, will either mistake or confound them. The viscera sometimes become scirrhous after active inflammation. The exterior glands are also frequently enlarged, and sometimes put on a scirrhous form of disease. There are also cold indolent tumors, which appear on the exterior of the body, over different parts. Such as the steatomatous, &c. However, there is so little analogy between these diseases, that the practitioner must be ignorant indeed; who would confound them.

The steatomatous, &c. may be extirpated without danger and to the relief of the patient, but the scrofulous cannot.

The bulk of the steatomatous, &c. is the principal inconvenience to be apprehended from them, for it is well known, that they sometimes grow to an enormous size without occasioning any alarming symptom ; but the scrofulous tumor is not the only inconvenience, that a patient labouring under this disease experiences. Indeed the tumor appears to be only an indication of a disease of the general system. Before any symptom of suppuration is discovered ; in fact as soon as the gland begins to enlarge, or even before there are symptoms of an enlargement, a disease of the general system is obvious. The skin becomes lax, the solids relaxed, there is a loss of appetite, dyspeptic symptoms, often supervene ; and these affections continue to increase, unless remedied, until a universal cachexy prevails, and the patient gradually declines until death removes him from this scene of affliction.

In scrofula in general the digitalis is certainly pernicious, but in that of the lungs it may be beneficial from the immediate influence it has on the pulse. But it should be used with great care, and if it does not reduce the action of the pulse, it should be discarded as injurious, as it reduces the tone of the general system. If it should reduce the action of the pulse, it will enable us to administer, more freely those medicines which we must have recourse to for the cure of this disease.

(To be continued.)

A Case of Artificial dilatation of the Female Urethra, &c. &c.

BY H. L. THOMAS, ESQ. F. R. S.

From the Medico-Chirurgical Transactions for 1809.

SUNDAY, April 26th, I was called into the country to see a lady, thirty-four years of age, and the mother of several children. On the morning of that day she had, without any assignable cause, laboured under a suppression of urine; as there was no regular assistance at hand, the husband (whose readings on this subject had been pretty extensive,) took upon himself to afford her relief. With this view he introduced an ivory earpicker into the meatus urinarius, and the water immediately flowed, but as the quantity evacuated did not quite equal his expectations, the instrument was introduced a second time, when it escaped from his fingers, and suddenly slipt, as he expressed it, with a "jerk into the body." When I saw her, six hours after, she was free from pain, and with no other unpleasant symptom than those which might be expected from the agitated state of her mind. Upon introducing the sound the extraneous substance was readily detected, and was as easily laid hold of by a pair of fine polypus forceps, but I found every attempt to extract it was attended with considerable pain, and followed by a slight discharge of blood, appearances which gave me reason to imagine that the instrument lay across the bladder, with its pointed extremities entangled between the fasciculi of muscular fibres, and that much mischief might be produced by any farther attempts at extraction. I therefore merely ordered an enema to be administered, and directed her to drink plentifully of diluting mucilaginous liquids; I also gave some directions respecting the position of the body at the time of evacuating the urine, which were to be such as to render the orifice of the bladder the most depending part.

On the Monday at two o'clock, I found that she had passed a restless night, and was much indisposed; pulse frequent, face flushed, and the tongue furred; a slight degree of tenderness was beginning to pervade the whole of the hypogastric region. This situation of the patient clearly pointed out the propriety of a speedy removal of the cause of irritation, and as I came provided with the bistoiré cachee, I proposed opening into the bladder immediately; this proposition was, however, absolutely rejected by the husband, from the knowledge he had of a case of lithotomy in a female, who ever afterwards suffered from an incontinency of urine.

With some hesitation, and I must confess with little prospect of success, I introduced a piece of sponge tent into the urethra, three inches long, and somewhat thicker than the full sized female sound, leaving directions for its removal at the end of two hours, or even sooner, if attended with much pain or distress. I took care to prevent the possibility of the sponge separating, and any part escaping into the bladder, by passing a strong string through its whole length secured at both extremities by a double knot.

In the evening I was informed that she had borne the tent for two hours, without any great degree of irritation taking place, and that during the whole time the urine gradually escaped. I sent another piece of the tent somewhat larger than the first, desiring it to be introduced at twelve o'clock the following morning, and allowed to remain in the urethra till I came to her at two. On my arrival I found her tolerable well, and upon withdrawing the tent I passed the forefinger of my left hand into the bladder, where I felt the ear-picker lying across the cervix. At first I conceived it impossible, without laceration to disengage it from the position in which it appeared so firmly fixed, but by turning my hand, and insinuating the point of the fore finger underneath, and towards the blunt extremity, it was readily dislodged, and escaped through the urethra, by the side of my finger. It measured three inches in length, one end being considerably more pointed than the other. I kept my finger engaged in the bladder for (I should guess) five minutes after the expulsion of the ear-picker, solely with a view of attending to the contractile force of that viscus. The sensations I experienced did not in any way accord with the ideas I had formed, for instead of my finger being firmly compressed, as I expected, by the sphincter muscle and bladder generally, I found every where nothing else than a soft pulpy yielding substance, totally insensible to any stimulus I could produce by the finger nail; this inability to contract, may perhaps be accounted for by the partial distention of the bladder for so long a time by the ear-picker. The same agent, however, cannot be assigned as the cause of the general relaxation which had taken place in the urethra, and which was so very complete, that before I withdrew my finger, I believe, had the case required it, both thumb and finger would have passed into the bladder without the smallest difficulty.

On the following day the patient was so well as to preclude even an excuse for any farther examination of the parts; but I was given to understand that the involuntary discharge of urine continued only six hours, and that now she possessed the full powers of expelling it without any other inconveniency than a slight degree of scalding in its passage along the urethra.

My view in laying this case before the Society is to show how readily, and with what little pain the female urethra will admit of dilatation. There is no novelty in the mode of accomplishing the object of distention. Near a century back it was proposed by Douglas; he not only recommended sponge for this purpose, but also dried gentian root, as being more gradual in its expansion, and better fitted for the purpose.

Mr. Bloomfield has given the case of a young girl where he effected the dilatation by introducing the cœcum of a small animal, in a collapsed state, into the bladder, filling it afterwards with warm water, by means of a syringe. This was gradually withdrawn as the cervix vesicæ opened, and in a few hours the dilatation was so far accomplished as to allow the calculus to pass through.

Why some of these methods have not been more generally adopted I cannot say, perhaps the incontinency of urine which occasionally has been observed to succeed to great distention of the urethra is the reason of their having been laid aside; with what propriety, however, may be questioned, for I believe it will be found that this unpleasant symptom frequently occurs after the operation of lithotomy, as it is now usually performed.

We have many well authenticated cases on record where calculi, of a size larger than a hen's egg, have been expelled from the bladder by the expulsive efforts of its own muscles. Heister has related from good authority several instances of the kind, a very remarkable case is also given by Dr. Molineux, in the early part of the Philosophical Transactions, where a stone was voided by a woman, "the circumference of which measured the longest way $7\frac{6}{10}$ inches, and round about where it was thickest $5\frac{3}{4}$ inches, its weight near $2\frac{1}{2}$ ounces troy.".

If these relations can be credited, and there is no reason why they should not, I can hardly conceive any cause in a young and healthy female subject, and where the bladder is free from disease, why a very large stone may not be extracted without the use of any other instrument than the forceps, the urethra having first been sufficiently dilated by means of the sponge tents; for this purpose the plates of the forceps need not be so thick and strong as those commonly employed.

It may be often observed, that muscular fibres, when once stretched to a certain extent, (i. e.) beyond their capability of immediately reacting, will admit of very considerable elongation without tearing, and yet shall, when the cause is removed, perfectly and very speedily recover their original powers. When the luxation of the femur takes place downwards, we observe the great gluteus muscle upon the stretch, almost to breaking: before the reduction can be accomplished the fibres will be still farther elongated, yet after this violence of extension, laceration of the fibres hardly ever happens, and when the muscles are again restored to their former situation, their functions become as perfect in a few days as if no injury had been sustained. That phincter muscles are possessed of similar powers, the above case will, I think, in great measure prove, in addition to which I beg leave to subjoin the following.

A gentleman of an inactive and sedentary disposition had for many years suffered from constipated bowels, which increased to that degree that the most active cathartics failed in producing the desired effect. By the advice of a practitioner, whom he consulted in Paris, he daily introduced into the rectum a piece of flexible cane (about a finger's thickness,) where it was allowed to remain until the desire for evacuating the fæces came on. This plan succeeded so well that for more than a twelve month he never had occasion to resort to any other means. One morning, being anxious to fulfil a particular engagement in good time, in his hurry he passed the stick farther up, and with less caution than usual, when it was suddenly sucked up into the body, beyond the reach of his fingers. This accident, however, did not interrupt the free discharge of the fæces, and the same evacuation regularly took place every day, whilst the stick remained in the gut. It was seven days afterwards when I first saw him; he was in a very distressed state, with every symptom of fever, tension of the abdomen, and a countenance expressive of the greatest anxiety. His relatives and friends were totally ignorant of the real nature of his case; and nothing less than the urgency of his sufferings, could ever have prevailed upon him to disclose it to me. Such were his feelings on the occasion, that a violent hysteric fit was brought on by the mere recital of what he termed his folly.

Upon examination with my finger, per anum, no part of the cane could be discovered; but one end of it was readily felt projecting (as it were) through the parietes of the abdomen, midway between the ilium and the umbilicus on the right side. The slightet presssure upon this part gave him exquisite pain.

After repeated trials I was at length enabled, with a bougie to feel one extremity of the stick lodged high up in the rectum; but without being able to lay hold of it with the stone forceps. To allay the irritation for the present, an emollient clyster, with tinct. opii. ʒij, was given, which passed without the least impediment, and did not return. On the next examination, two hours after, I found the phincter ani considerably dilated, and by a continued perseverance to increase it, the relaxation became so complete, that in about twenty minutes I was enabled to introduce one finger after the other, until the whole hand was engaged in the rectum.

I found the end of the stick jammed in the hollow of the sacrum, but by bending the body forward it was readily disengaged, and extracted. Its length was nine inches and an half, with one extremity very ragged and uneven.

For several days after, the situation of the patient was highly critical, the local injury, joined to the perturbation of his mind, brought on symptoms truly alarming. At length I had the satisfaction to witness his complete recovery; and he has ever since (more than two years ago) enjoyed good health, and the regular action of the bowels, without the assistance of medicines, or any other aid.

Haslam on the Locked-jaw in Horses.

Baltimore, 2d July, 1811.

To the Editor of the Lycaeum.

SIR,

I HOPE it will not be considered disrespectful to the faculty, or presumption in me, to state a few facts and opinions respecting the *locked-jaw* of horses. This useful animal is much oftener affected with tetanus than is generally supposed, and it has almost in every instance ended in death. Although I think I can say without vanity, that I have been successful in other diseases of the equine race, I candidly confess I never succeeded in a case of this dreadful malady till you recommended the use of calomel. I had prescribed opium, brandy, camphor, spirit of turpentine, and many other strong stimulants, but to no good purpose.

As all your readers may not be acquainted with the symptoms of this disease in horses, I will state them as they appeared to me in those cases that fell under my observation.

The nictating membrane (which in a state of nature is but partially to be seen) becomes conspicuous; the nose is pointed horizontally, and the tail raised considerably and is turned a little to one side. The horse ruminates his food like a cow, scarcely separating his jaws, and his walk is much changed; both his fore and hind legs being much farther apart than usual. His flesh becomes very hard, and at last his jaws are completely locked. This is commonly the last symptom, but in some cases all these symptoms appear nearly at the same time. When this is the case I consider it as an unfavourable sign; for the slower the symptoms succeed to each other the less dangerous the disease and vice versa, for when the affection of the jaws soon succeeds the other symptoms there is the greatest danger.

One of the greatest impediments to the cure of this disease is the vulgar error of supposing that the horses have what are called the *hooks*, an opinion that originated in ignorance, because it has no foundation in nature. To prevent the cruel operation of cutting for this supposed disease, (which would be worse then the disease if it existed at all) I shall give a description of this part of the eye.—The horse like all other animals being liable to the introduction of foreign matters into the eye, and not being favoured like man with hands or other means of removing them, has been bountifully furnished by nature with a nictating membrane. This is of a strong caitilaginous nature, and is put in motion by the retractor muscle drawing the eye into the orbit, by which motion this membrane is brought over the globe of the eye. Thus, by the motion of the eye it serves to cleanse or wipe off any offensive matter that may have been introduced.

I have met with some well informed people as to other subjects who believed that this membrane had arisen there in the course of a few days, because they were not acquainted with the causes that rendered it more visible than usual. There are many such causes, but more especially the two following. 1st. Inflammation in the eye, which when stimulated by the rays of light is drawn into the orbit, leaving the membrane partially exposed. 2d. Spasms of the muscles of the eye or parts adjacent which produce the same effect, and is the reason why, the tetanus is so often supposed to be the hooks which have no existence. This mistake is often fatal; for I have sometimes been unable to persuade the owners of horses of their deception and the cartilage has been cut out to the destruction of the poor animal. Such is the prevailing opinion on this subject, that as soon as a horse is sick of any complaint which they do not understand (and this is generally the case) they begin to look for something external that will enable them to account for the symptoms, and this is often very striking in the eye, because in many diseases this membrane is more than usually obvious for the reasons already assigned. The operation is immediately performed, and if the horse does not die of inflammation or a locked jaw, he generally becomes blind, and will be so for life.

Of the causes that give rise to this disease in horses I have observed injuries of the feet (more especially from nails) to be the most common. There seems to be something in iron nails, that oftener occasions tetanus than any thing else. Eight out of the last ten cases under my care were brought on by this cause. It is nevertheless sometimes brought on by wounds from other causes. I have seen a case produced by docking the tail, and I cannot forbear to observe here that I think strong stimulating dressings after certain operations, tend to produce this effect. I have also seen it brought on by an operation which was followed by a high degree of inflammation. On the 7th of June, 1810, I removed a large tumor from the scrotum of a mule. It had become so large as to be very inconvenient. After the operation there was a very free discharge and every thing looked well; but on the 19th, I was sent for, and found that he laboured under a complete tetanus. Although it had appeared only a few hours, he died before morning. I opened the body the next morning and found, that the inflammation had communicated to the intestines and stomack, and that he had died of mortification.

I have conceived that there are two species of tetanus very different from each other; the one evidently arising from great and general debility giving rise to spasms, and the other originating from excess of inflammation requiring evacuations for its cure. The above case shews at least that either the tetanus arose from inflammation, or was present at the same time, and I think we must conclude they were connected as cause and effect. Nor is this the only case that induces me to draw such a conclusion. On the 26th of February, 1810, I opened the body of a horse, that had died of tetanus, and had been cut for the hooks. I found the right lobe of his lungs in a high state of inflammation as well as the whole intestinal canal. In certain cases of this disease all the common symptoms of fever are to be met with. I have for these reasons long viewed the disease as both acute and chronic and have thought that the slow or chronic is the least to be dreaded. I imagine that the disease proceeds to a more rapid termination, when it proceeds from fever and the reverse. It is remarkable in tetanus from wounds that the wounded part always heals long before the symptoms of locked jaw appear, in many from five or six days to two and even three weeks afterwards.—I have always noticed that when the spasms were communicated to the intestines that the chance was a bad one, and that an obstinate constipation was equally unfavourable. A free passage, especially when not produced by art, is I believe, always favourable.—Since you recommended the use of mercury, I have cured three cases out of five, and I am of the opinion, that the fourth would have done well, had not the owner of the horse contrary to my advice, permitted the operation for the hooks to have been performed. The fifth was one of those desperate cases, and so far advanced, that I think no remedy could have had any effect. I could not perceive that the mercury had any effect upon the excretions, or any other, except that of relaxing the spasms in a few hours after it was given. The glands were not affected that I could perceive, as no part of the mouth, fauces or tongue shewed any marks of its action. The intestines were little, if at all affected, as no purgative effects were perceptible.*

Perhaps it would be useful to state a case exhibiting in general the mode of treatment I have used in those cases where mercury proved successful.

On the 22d March, 1810, I was called to see a horse supposed to have had the hooks, and as the owner pretended to no knowledge of the disease, he had through the persuasion of others suffered the operation to be performed several days before I saw him. Upon enquiry I found that a bone had entered one of his feet about three weeks before; but had been extracted a day or two afterwards, and left little or no lameness. For this reason those who saw him could not be persuaded that the disease could have arisen from this cause. It was easy to be perceived that the symptoms of locked jaw were increasing, but as they had proceeded gradually my prospect was pleasing. The wounded part was dilated immediately and dressed with cantharides and spirit of turpentine. The symptoms did not abate and the evident marks of strong spasmodic action rendered something farther necessary. I with great difficulty introduced into the mouth six drams of calomel. On the 23d the calomel was repeated. 24th calomel repeated, but no appearances of relief. 25th calomel repeated, and the spasms less frequent and of shorter duration. 26th spasms relieved. This morning he passed from the rectum a small watery discharge, but nothing like a free or active purgation. 27th, he is better, and can distend his jaws considerably, but has no appetite. I gave an injection of corn meal gruel. 28th, purges a little, and the injection was repeated. 29th, no more passage from the intestines. His jaws quite free, and appetite returning. He soon recovered, and is now a very fine animal.

---

* I should have remarked that the intestines almost in every instance very soon partake of the spasmodic affections, and that this is a very hard state to overcome.

Your obedient servant,

JOHN HASLAM.

……

Remarks on Hydrophobia, and a new method of using the Caustic by H. Wilkins, M. D. in a letter to the Editor.

SIR,

AS I conceive that any improvement in the manner of treating hydrophobia, will be acceptable to the public, I shall make known through the Lycæum, the simple and efficacious method I have used, and recommended for some years, to eradicate the dreadful poison that occasions it. A method not altogether new in substance, though it be new in form, and if applied with a little attention, would reduce this once tremendous disease to the lowest degree on the scale of mortality.—Before disclosing my plan, permit me to make a few observations, that relate to the great importance of destroying the virus in its first lodgement; for I consider all other plans of prevention as merely experimental, and deviating from true wisdom.

As to the medicines, called specifics, or family medicines, I regard them in toto as the base offspring of ignorance, or avarice, and not worthy an examination: a gloomy experience having long since decided against them. With regard to these it is worthy of remark, how error begets error. I have known a kind of hawk-weed, (Hyoseris Amplexicaulis,) given with the greatest applause under an idea that it was the litchen caninus, though not even belonging to the same class of plants; by which we have gained the knowledge that H. A. is a diuretic and may be safely given in as large doses as the litchen, but surely there never was a human being saved from hydrophobia by it!—And I have no doubt but many a little running plant has been swallowed for the famous anagallis, hereafter to be remembered for having arrested the attention of the legislature of Pennsylvania, more than for all its pretended cure.

The well authenticated histories of hydrophobia leave no room to doubt, that scarce any period, within a year from the infliction of the wound, is too * long for a local affection to exist nor too short † for it to expand into the dreadful features that characterize the disease. How far this variation depends upon the irritability of the general habit, the local irritability of the part wounded, or the sympathetic disposition of the wounded organ with the parts afterwards associated in diseased action; or the disposition of the wounded part to suppurate; I will not take upon me to determine: but I believe them concerned in determining, both the certainty and the time of the disease.

---

* Dr. Vaughans cases and observations on hydrop. page 22, a case after nine months.

† Dr. Grays case on India, where death followed in the course of the same day.

When we consider a period so long as nine months from the insertion of the virus to the production of the disease, with an intervening state of apparent health; as in the case alluded to above, the least consideration will lead us to the conclusion, that the local disease must be first formed about the wounded part; and that, the disease remained there in a state of subexistence: for we cannot suppose that a drop of poisoned saliva could remain unabsorbed so long, nor remain without generating new matter; nor that it could in a slight skin wound, (which will often produce the disease,) send forth irritations, after the manner of tetanus, to produce such a set of symptoms, as characterize the disease in question. It must therefore be gaining strength by generating new matter, till it be sufficiently powerful to derange all the natural actions.—With respect to the poison laying local and first infecting the place of deposit, though not peculiar to this disease, it may be difficult to explain satisfactorily the pathology, yet, on this dark and and intricate subject, I have often pacified, if not satisfied my mind by adverting to the doctrine and to the fact, which takes place in vegetation. It is well known, that after cutting down certain vegetables, as grasses for instance, another kind will spring up, and in some instances fir-trees have been raised, by cutting off the incumbent timber, where they had been unknown for a long series of years. There can be no doubt, but that a degree of vegetation had constantly gone on, though not in full form, owing to the reigning crop which had kept them in a state of subexistence: certain fibrillæ and gangliæ being discoverable under ground in the case of the firs alluded to above.

In this way I conceive does vigorous health keep under various morbific seeds, which are ready to germinate upon the least overthrow of the general healthy state, and then put on the full characteristic form by which we know and name them.

A question might be here propounded whether the materies of hydrophobia does dot act simply from irritation as a wound from a rusty nail produces lock-jaw?—There seems to be a possibility of this, when we consider the rapid movement of the case in India by Dr. Gray as quoted above; but I think it inquestionable, that it gains strength and quantity at the part, before it is discharged in sufficient power to cope with and overcome the natural actions; for it is by altering these and instituting new actions, that the disorder is formed.

Was it not for the power of communicating the identical desease, we might be greatly puzzled to determine whether there was any morbific matter, or any difference in the mode of action from that in lock-jaw; viz. by simple irritation at the wound, affecting certain parts, according to the relation that such irritation bore to such or such parts of the system.—I would presume to judge that in so rapid a case as the one alluded to above, there could scarcely be any more morbific matter formed than what was first inserted.

After what has been said, I ask the question, how can the mind be turned from the purpose of making a local application, that will destroy the first diseased part, and perhaps decompose the poison itself, so that if left in the wound it might not act? There an application is always proper, for it can never do harm, and if the symptoms are not yet discoverable that characterize a general disorder, we are morally sure of success.

Here the time that the disease takes to form itself, is of no consequence, unless it be the longer the more favourable.—It is worth while to examine well a method of this kind and for a fair elucidation,. to compare it with what is called a specific; because I do not insist, that such a discovery may not be hereafter made. E. G.—A and B are bitten at the same time, and by the same dog—A. in the common course of things would have the disease in nine days, B. not till nine weeks or months. Let them both be treated at any anterior time with the caustic (hereafter recommended) and both are surely cured.—But let them take the internal specific for a month: A. is cured, and B. is supposed to be also cured, but the disorder is yet local, when the specific is laid aside, and behold at an unexpected time, a time of ideal security, the disorder spreads insidiously forth in all its terrible forms to invalidate the cure in A. and destroy B. What is now to be done? Another specific is wanted; a specific cure to aid the specific preventive! Let credulity itself blush for such hopeless contingencies, and adopt sound sense. But should not the specific medicine have been continued a longer time? Yes, a man may eat medicine for a year, but it would only keep him sick, and of course in constant fear, if it had any powers in it; and the physician must be ignorant indeed that does not know the difficulty of enforcing a long continuance of any drug.

To delay no longer, the method I recommend is as follows—Take ℥ij. of hard vegetable caustic or caustic of potash, beat it fine, drop it in water and rub it till it becomes of the consistence of paint, such as is used for house painting. Dip a camel's hair pencil, or if that cannot be procured, a piece of fine linen rolled up hard, in the manner a mop is made, and thrust this paste into every sinus of the wound. Wait about one minute, when a shining granulous appearance shews that it has corroded the parts, then with a small syringe force in some weak, warm vinegar, which will clean out the wound and destroy the caustic. If the wound be considerably enlarged, no more need be applied, but if it is not, apply it again as at first and wait a little longer, when if the wound is cleaned out as before, it will be about as large again as it was before the application was made. This cleaning out of the wound may be notwithstanding my former supposition, all important, for what will it avail to destroy the part and leave the matter there? This points out the excellency of common caustic over lunar caustic. Nothing farther remains than to dress the wound with a good suppurative salve, such as merc: præcip: rub: levigat: ʒiij: cum unguento Basilic: ℥i.

The pain produced by the caustic paste is very trifling; children bear it without crying. In one instance I applied it to twelve wounds on a boy of ten years of age and he seldom changed countenance or shewed signs of pain. In this case I had full proof of the success of the caustic; that it was a genuine case, there remains no doubt, for a horse and cow were bitten at the same time by the same dog, and both these died, whilst the boy is alive and well.

I could enumerate many instances where I have recommended, or applyed the caustic with success, but I trust the method wants no supports. It is safe, easy and sure. I have mentioned above, that exclusive of removing the diseased part, there was a strong probability that the materies was decomposed by the chemical action of the caustic, so as to be inert, even if left in the wound. This probability (though never to be trusted) is farther confirmed by the digestive powers having at times a similar effect.

I will relate a well authenticated case, as it is singular in the extreme, that may throw light on this, as well as on other points of this interesting disorder. A mad fiste went to a farm five miles from Baltimore, and there bit one of the domestic dogs, after which the fiste was killed. A very poor dog belonging to some of the blacks on the same farm, was seen by two gentlemen who had visited the house, eating the dead fiste. They were shocked, and attempted to drive him off, but he seemed so unwilling to leave it, that they left him. In a short time they returned by the same road, and found that the whole head had been eaten off. I have made enquiry (having daily opportunities) and find that the dog bitten died mad, but that the carnivorous dog is actually in better health.

The case of Admiral Rowley's son, might be brought as an objection that caustic was not infallible. The lip is certainly a bad part of the mouth, and in such cases a tooth may pass inside and not be noticed. Lunar caustic, is, moreover, very unfit for the purpose; and the person, recommended by Mr. Hunter, might have been very much off his guard by the manner in which the youth was attacked *by disturbing a* *strange dog at his meal, and unknown to be diseased by his owner.*[*]

When the mouth is bitten, as is often the case in children who are generally crying at the moment the attack is made, the gums, tongue and every contiguous part should be well examined.

Although I have avoided giving a description of the usual symptoms that characterize hydrophobia, as being useless at this time; yet I conceive it highly proper to notice any symptoms that have escaped general notice; and I would here note particularly, the intoxicated deportment and appearance about the eyes and a croup like sound in barking, as symptoms of hydrophobia. Hence the reason why patients in this disease have been said to have barked like dogs; for there seems to be an affection of the trachea, or gluttis which alters the sound in both the human and canine species.

Before closing this, I would caution every person against considering the dread of water as unequivocal. I have seen two instances where they drank freely and yet communicated the disease.

I do not think it proper whilst we are so far from perfection in the knowledge of this disease, to alter the general character, because in a few instances it is defective, and in this I am sanctioned by a great character, no less than Linneus.

Hoping that these observations may be useful, and lead to an uniform plan of prevention,

I remain, sir, yours, &c.

H. WILKINS.

P. S. Would not the paste caustic answer completely in shallow fistulas to remove the callous edges, and be a very easy mode of operating both to physician and patient?

---

[*] Annual Register, January 19, 1805.

MEDICAL

And Philosophical Intelligence.

AN ACT

FOR FOUNDING A MEDICAL COLLEGE

In the City or Precincts of Baltimore, &c. &c.

……

An Act for founding a MEDICAL COLLEGE, *in the city or precincts of Baltimore, for the instruction of Students in the different branches of Medicine.—Passed November Session*, 1807.

WHEREAS it appears to the General Assembly, that many benefits would accrue, not only to the State of Maryland, but to many other parts of the United States, from the establishment of a Seminary for the promotion of Medical knowledge, in the city of Baltimore—Therefore,

2 *Be it enacted by the General Assembly of Maryland,* That a College for the promotion of Medical knowledge, by the name of "THE COLLEGE OF MEDICINE OF MARYLAND," be established in the city or precincts of Baltimore, upon the following fundamental principles, to wit: The said College shall be founded and maintained forever upon a most liberal plan, for the benefit of Students of every country and every Religious denomination, who shall be freely admitted to equal privileges and advantages of education, and to all the honors of the College, according to their merit, without requiring or enforcing any religious or civil test, or

urging their attendance upon any particular plan of religious worship or service; nor shall any preference be given in the choice of a President, Professor, Lecturer or other Officer of the said College, on account of his particular religious profession; but regard shall be solely paid to his moral character, and other necessary qualifications to fill the place for which he shall be chosen.

§ 3. *And be it enacted*, That the Members of the Board of Medical Examiners for this state, for the time being, together with the President and the Professors of the said College, and their successors, shall be and are hereby declared to be one Community, Corporation and Body Politic, to have continuance forever, by the name of the "*Regents of the College of Medicine of Maryland.*"

§5. *And be it enacted*, That the said Regents and their successors, by the same name, shall be able and capable to enjoy to them and their successors in fee or for any less estate or estates, and lands, tenements, annuities, provisions or other hereditaments within this state, by the gift, grant, bargain, sale, alienation, enfeoffment, release, confirmation or devise of any person or persons, bodies politic or corporate capable to make the same, and such lands, tenements, rents, annuities, pensions, or other hereditaments, or any less estates, rights or interests of or in the same, at their pleasure to grant, alien, sell und transfer in such manner and form as they shall think meet, and convenient for the furtherance of the said College; and, also, that they may take and receive any sum or sums of money, and every kind, manner or portion of goods and chattels, that shall be sold or given to them, by any person or persons, bodies politic or corporate, capable to make a gift or sale thereof, and employ the same towards maintaining the said College, in such manner as they shall judge most necessary and convenient for the instruction of Students in Medicine and the sciences connected with it.

§ 5. *And be it enacted*, That the said Regents and their successors, shall be able in law to sue and be sued, plead and be impleaded in any court or courts, before any judge, judges, or justices within this state and elsewhere, in all and all manner of suits, pleas, causes, matters and demands of whatsoever kind, nature or form they be, and to do all and every other matter and thing hereby contemplated to be done, in as full and effectual a manner as any other person or persons, bodies politic or corporate, in like cases may or can do.

§ 6. *And be it enacted,* That the yearly value of the messuages, houses, lands, tenements, rents, annuities, or other hereditaments and real estate of the said College and Corporation (exclusive of the lot and buildings occupied by the institution) shall not exceed thirty thousand dollars; and all gifts to the said College and Corporation, after the yearly value of their estates shall amount to thirty thousand dollars aforesaid, and all bargains and purchases to be made by the said Corporation, which may encrease the yearly value of said estate above or beyond the amount aforesaid, shall be absolutely void and of none effect.

§ 7. *And be it enacted,* That the said Regents and their successors, shall have full power and authority to have, make and use one common and public seal, and likewise one privy seal, with such devices and inscriptions as they shall think proper, and to ascertain, fix and regulate the uses of both seals, by their own laws, and the same seals, or either of them to change, break, alter and renew at their pleasure.

§ 8. *And be it enacted,* That the said regents shall have full power and authority to appoint a President of the College, who shall preside at their meetings, and perform such other duties as may be assigned to him; and in his absence to appoint a Vice President, which latter shall always be one of their own body.

§ 9. *And be it enacted,* That the said Regents shall from time to time, and at all times hereafter forever, have full power and authority to constitute and appoint, in such manner as they shall think best and most convenient, Professors of the different branches of Medicine, for instructing the Students of said College, by regular lectures upon every part of that science, who shall be severally stiled Professors of such branches as they shall be nominated and appointed for, according to each particular nomination and appointment; and also to constitute and appoint in like manner, Lecturers upon the sciences subservient to or connected with Medicine, who shall be severally stiled Lecturers on such sciences as they shall be appointed for; and the said Professors and Lecturers so constituted and appointed from time to time, shall be known and distinguished forever as one learned body or faculty, by the name of the *Medical Faculty of the College of Medicine of Maryland*, and in that name shall be capable of choosing their own Dean of Faculty and of exercising such powers and authorities as the Regents of the said College, and their successors, shall by their ordinances think necessary to delegate to them, for the instruction, discipline and government of the said institution, and of all Students, Officers and Servants belonging to the same, provided that nothing be done, in virtue of this, contrary to the constitution and laws of this State, or to the constitution and laws of the United States.

§ 10. *And be it enacted*, That the said Regents and their successors shall meet at least once a year in stated annual meetings, to be appointed by their own ordinances and at such other times as by their said ordinances they may direct, in order to examine into all matters touching the discipline of the institution, and the good and wholesome execution of their ordinances; and that the said Regents, when duly assembled, shall have full power and authority to make their own rules of proceeding and to make fundamental ordinances for the government and discipline of the College, and to appoint the necessary officers of the same: in all which meetings a majority of the whole number of Regents shall be a quorum to do any business, except to vacate the seat of the President, Professor or Lecturer, for which purpose the consent of two thirds of the whole number of Regents shall be necessary.

§ 11. *And be it enacted*, That the Medical faculty of the said College, shall hold one term in each year, which shall commence on the first Monday in November, in every year, and shall continue not less than four, nor more than six months, as may be regulated by the ordinances of the College; within which period all the Lectures to be delivered in the said College, shall be given, except the Lectures on Botany, and such other Lectures as the Regents shall judge will be delivered with more advantage at a different season.

§ 12. *And*, for animating and encouraging the Students of said College, to a laudable diligence, industry and progress in Medical science, *Be it enacted*, That the said Regents and their successors, shall, by a written mandate under their privy seal and the hand of their President or Vice President, have full power and authority to direct the Medical faculty of the said College, to hold Public Commencements, either on stated annual days, or occasionally, as the future ordinances of the said institution may direct; and at the said Commencements to admit any of the Students of the said College, meriting the same, (whose names shall be severally inserted in the said mandate) to the office and profession of Surgeon, or to the degree of Batchelor or Doctor of Medicine; and it is hereby enacted, that the President or Vice-President shall make out and sign with his name, diplomas and certificates of the admission to such offices or degrees, which shall be sealed with the Public or Greater Seal of the College or Corporation, and delivered to the Graduates as honourable and perpetual testimonials of such admission; which diplomas or certificates, if thought necessary for doing greater honour to such graduates, shall also be signed by the names of the different Professors or Lecturers, or as many of them as can conveniently sign the same: *Provided always*, That no Student or Students, within the said College, shall be admitted to any such offices or degrees, or have their names inserted in any mandate for that purpose, until such Students shall have been first duly examined and thought worthy of the same at a Public Examination of Candidates, to be held previous to the day of Commencement, in the said College, by and in the presence of the Regents and other persons, choosing to attend the same; and shall also have undergone such previous private examinations, and have performed such exercises as shall be prescribed by the future ordinances of the College: *And provided further*, That no Student or Students in the said College, shall be admitted to the degree of Doctor of Medicine, unless he or they shall have attended Lectures in the said College during two Terms, and shall, during that period, have attended each of the Lectures prescribed by the ordinances, for at least one term: and shall also have written and caused to be printed, a thesis or theses in the Latin or English language, and shall publicly defend the same on the day of Commencement: But the Regents of the College shall, at any time, have power to consider the having attended Lectures in any other Medical Seminary of established reputation, and for equal space of time, as equivalent to having attended one of the Terms above prescribed.

§ 13. *And be it enacted*, That the Regents shall, at any time, have the power of conferring the honorary degree of Doctor of Medicine on any Physician, who has practised Physic for twenty years, within the State of Maryland; and of conferring the honorary degree of Batchelor of Medicine on any one who has practised Physic for ten years within the same.

§ 14. *And be it enacted*, That every Licentiate of the Board of Medical Examiners, who shall have practised Physic for five years within this State, shall have a right to demand and receive from the College aforesaid, a Surgeon's Certificate, free of all expense, except the sum of one dollar to the Register or other such officer of the College for his trouble in making out the same.

§ 15. *And be it enacted*, That the right of the said College to confer degrees, shall not take place until Professors shall have been appointed, and one course of Lectures delivered upon the several branches of Anatomy, Chemistry, Materia Medica, and the practice of Physic: *Provided*, That nothing in this act shall be construed to prohibit the said Regents from conferring, at any time, the honorary degrees before mentioned; or from admitting at any time, to the degree of Doctor of Medicine, such persons as may be appointed Professors in the College, on their performing such Exercises and duties as may be prescribed by the ordinances.

§ 16. *And be it enacted*, That until further arrangements be made by the Regents of the said College, *John B. Davidge*, *M. D.* and *James Cocke*, *M. D.* shall be joint Professors of Anatomy, Surgery and Physiology; *George Brown*, *M. D.* shall be Professor of the Practice and Theory of Medicine; *John Shaw*, *M. D.* shall be Professor of Chemistry; *Thomas E. Bond*, *M. D.* shall be Professor of Materia Medica, and *William Donaldson*, *M. D.* shall be Professor of the Institutes of Medicine; and the said Professors, or any three of them, shall appoint the time and place of the first meeting of the Regents of the said College, giving one week's notice of it in two newspapers published in the city of Baltimore.

§ 17. *And be it enacted*, That all Students who matriculate in the said College, previous to the first day of January, and attend any of the Lectures therein, to the end of the course, shall be considered as having completed a Term.

§ 18. *And be it enacted*, That the *Medical and Chirurgical Faculty* of the State of Maryland, shall be considered as the patrons and visitors of the said College; and their President, for the time being, shall be Chancellor of the College; and the Medical Faculty of the said College, shall give in to the said Medical and Chirurgical Faculty, at each of their biennial meetings, a report of the progress of learning, in the said College, and of such other particulars as they shall think fit to communicate.

§ 19. *And be it enacted*, That in case, at any time hereafter, through oversight, or otherwise through misapprehension and mistaken construction of the powers, liberties and franchises in this Charter or Act of Incorporation, granted or intended to be granted, any ordinance should be made by the said Corporation of Regents, or any matters done and transacted by the Corporation, contrary to the ten or thereof—all such ordinances, acts and doings shall, of themselves, be null and void: Yet they shall not, in any Court of law, or by the General Assembly, be deemed, taken, interpreted or adjudged into an avoidance or forfeiture of this Charter and Act of Incorporation; but the same shall be and remain unhurt, inviolate, and entire, unto the said Corporation of Regents, in perpetual succession; and all their acts, conformable to the powers, true intent and meaning hereof, shall be and remain in full force and validity, the nullity and avoidance of such acts to the contrary notwithstanding.

§ 20. *And be it enacted*, That this Charter and Act of Incorporation, and every part thereof, shall be good and available in all things in the law, according to the true intent and meaning thereof; and shall be construed, reputed and adjudged, in all cases, most favourable on behalf and for the best benefit and behoof of the said Regents and their successors, so as most effectually to answer the valuable ends of this Act of Incorporation towards the general advancement and promotion of Medical knowledge.

I CERTIFY, That the aforegoing is a true copy from the original, passed both branches of the Legislature, on the eighteenth day of December, one thousand eight hundred and seven.

Test. JOHN BREWER, Clk. H. Del.

College of Medicine, of Maryland.

Courses of Lectures will be delivered in this Institution next winter on the following subjects:

1. On Anatomy, Surgery, and Physiology, by John B. Davidge, M. D. and James Cocke, M. D.
2. On the Practice and Theory of Medicine, by Nathaniel Potter, M. D.
3. On Chemistry, by Elisha De Butts, M. D.
4. On Materia Medica, by Samuel Baker, M. D.

The courses of Lectures will commence on the first Monday in November, 1811.

By order of the President,

JAMES COCKE, Sec'ry.

………

Licentiates admitted to practice Physic and Surgery, in the State of Maryland, at the meeting of the Board in June, 1811.

Jonathan Waters,
Robert Dawson,
John S. Stockett,
Henry Stonestreet,
Stanislaus Coomes,
Benedict I. Semmes,
I. B. Taylor,
William N. Luckey,
Corbin Amos,
Charles D. Bruce.

………

At a meeting of the Regents of the University, held at New York, pursuant to adjournment, in the Senate Chamber, on the 1st *of April,* 1811:

Resolved, That the following appointments be and hereby are made in the College of Physicians and Surgeons in the city of New York, to wit:

Samuel Bard, M. D. President,
Benjamin De Witt, M. D. Vice President.

Wright Post and John Augustine Smith, Joint Professors of Anatomy, Surgery and Physiology.

David Hosack, M. D. Professor of the Theory and Practice of Physic and Clinical Medicine.

William James Mac Neven, M. D. Professor of Chemistry.

John R. B. Rodgers, M. D. Professor of Obstetrics, and the Diseases of Women and Children.

Edward Miller, M. D. Professor of Therapeutics, and Clinical Medicine.

William Hamersley, M. D. Professor of the Institutes of Medicine.

Samuel L. Mitchell, M. D. Professor of Natural History.

James S. Stringham, M. D. Professor of Medical Jurisprudence.

Archibald Bruce, M. D. Professor of Mineralogy and Pharmacy.

John D. Jaques, M. D. Treasurer.

John W. Francis, Register.

And that all former appointments made in the said College of Physicians and Surgeons be, and the same are hereby revoked.

A true extract from the minutes,

FR. BLOODGOOD, Sec'ry.

Translated from the Bulletin de la Societe de la Medicine.

DEATH OF FOURCROY.

THE medical faculty was yet deploring the loss of several of its professors, when a new object of affliction was added to the mass of regrets, so profound and so recent. *Count Fourcroy* died suddenly, on the morning of the 16th December, 1809, of an apoplectic fit; at the tomb, *D. Thouret*, in the name, and as Dean of the Faculty of Physic, pronounced the following discourse:

Profound grief reigns in this assembly; from every side breaks forth regrets; what bounds, what limits can be given to them!

What a man hath death torn from us? What talents are this day swallowed up in the grave? A glory earlier than usual, had announced what he was going to be. It seems as if fate had foreseen that nature would circumscribe his career, and had willed him a recompense. Scarcely had he left the benches—what do I say—he still sat among us—when we already marked the place that he was destined to hold among the most illustrious men. In the first bloom of his years in the midst of his fellow students, he made attempts to excel in the science of that professorship which he has carried to so high a degree of perfection.

A pupil of the celebrated *Bucquet*, he soon became his rival; and the teaching of chemistry which that learned professor, as it were, bequeathed him, lost nothing of the splendour which its first master had given it.

Talent in him was animated with that strong passion which alone can produce entire success. He sought all opportunities of displaying it, at the same time that all the establishments vied with each other in affording them to him, and in soliciting his support.

Who can properly paint that indefatigable activity which rendered him equal to all his duties to all the labours of his numerous chairs? Who can describe that astonishing care, which enabled him in each to exhibit science in becoming colours? Great and sublime in general chemistry; fruitful and rich in its application to the arts; bright with creations in animal chemistry! With what superiority of views were all objects treated! With what charm and power of elocution were they displayed! In what order, in what number, in what imposing masses were they grouped.

What crouds pressed to his learned lectures! How were all the avenues to our ampitheatre besieged! How were our porches covered with the multitude of our students!

That elegant voice is silent! It is forever shut! but the glory of the illustrious professor will never be diminished. It will live in our remembrance, in the memory of his cotemporaries, it will be transmitted to the remotest posterity, by the faithful tradition of the grateful school. And how can this glory be eclipsed? As distinguished in the sciences by the talent of writing as by the gift of speech, he has left imperishable monuments of his immense knowledge, and of his uncommon abilities. What can be placed higher than the numerous series of papers, in which so many objects of anatomy, of physiology, of natural history, and of chemistry, have been ably treated? What more durable monument can be erected in honour of the sciences, than that vast system of chemical information, in which the knowledge and the eloquence of the author shine with equal lustre?

He was the *Bergman* of France. He was more than *Bergman*. Happy would it be if for the sake of the worthy by celebrating his memory, he had our immortal *Vicq d' Azyr* to pronounce his panegyric!

THE
BALTIMORE
Medical and Philosophical Lycæum.

Vol. I. No. III.

FOR JULY, AUGUST AND SEPTEMBER.....1811.

Researches on the Oxymuriatic Acid, its nature and combinations; and on the Elements of the Muriatic Acid. With some experiments on Sulphur and Phosphorus, made in the Laboratory of the Royal Institution.[*]

By H. DAVY, Esq. Sec. R. S. Prof. Chem. R. I. F. R. S. E.

From the London Philosophical Magazine.

THE illustrious discoverer of the oxymuriatic acid considered it as muriatic acid freed from hydrogen,[*] and the common muriatic acid as a compound of hydrogen and oxymuriatic acid; and on this theory he denominated oxymuriatic acid, dephlogisticated muriatic acid.

M. Berthollet,[*] a few years after the discovery of Scheele, made a number of important and curious experiments on this body; from which he concluded, that it was composed of muriatic acid gas and oxygen; and this idea for nearly twenty years has been almost universally adopted.

Dr. Henry, in an elaborate series of experiments, made with the view of decomposing muriatic acid gas, ascertained that hydrogen was produced from it by electricity; and he attributed the phænomenon to water contained in the gas.*

---

[*] Mem. Acad. Stockholm for 1774, p. 94.

[†] Journal de Phisique, 1785, p. 325.

In the Bakerian lecture for 1808, I have given an account of the action of potassium upon muriatic acid gas, by which more than one-third of its volume of hydrogen is produced; and I have stated, that muriatic acid can in no instance be procured from oxymuriatic acid, or from dry muriates, unless water or its elements be present.

In the second volume of the *Memoires d'Arcueil*, M. M. Gay Lussac and Thenard have detailed an extensive series of facts upon muriatic acid and oxymuriatic acid. Some of their experiments are similar to those I have detailed in the paper just referred to; others are peculiarly their own, and of a very curious kind: their general conclusion is, that muriatic acid gas contains about one quarter of its weight of water; and that oxymuriatic acid is not decomposable by any substances but hydrogen, or such as can form triple combinations with it.

One of the most singular facts that I have observed on this subject, and which I have before referred to, is, that charcoal, even when ignited to whiteness in oxymuriatic or muriatic acid gases, by the Voltaic battery, effects no change in them, if it has been previously freed from hydrogen and moisture by intense ignition in vacuo.

This experiment, which I have several times repeated, led me to doubt of the existence of oxygen in that substance, which has been supposed to contain it above all others in a loose and active state; and to make a more rigorous investigation than had been hitherto attempted for its detection.

If oxymuriatic acid gas be introduced into a vessel exhausted of air, containing tin; and the tin be gently heated, and the gas in sufficient quantity, the tin and the gas disappear, and a limpid fluid precisely the same as Libavius's liquor, is formed: it occurred to me, that if this substance is a combination of muriatic acid and oxyde of tin, oxyde of tin ought to be separated from it by means of ammonia. I admitted ammonical gas over mercury to a small quantity of the liquor of Libavius; it was absorbed with great heat, and no gas was generated; a solid result was obtained, which was of a dull white color; some of it was heated, to ascertain if it contained oxyde of tin; but the whole volatalized, producing dense pungent fumes.

---

* Philosophical Transactions for 1800, p. 191.

Another experiment of the same kind, made with great care, and in which the ammonia was used in great excess, proved that the liquid of Libavius cannot be decompounded by ammonia; but that it forms a new combination with this substance.

I have described, on a former occasion, the nature of the operation of phosphorus on oxymuriatic acid; and I have stated that two compounds, one fluid and the other solid, are formed in the process of combustion, of which the first, on the generally received theory of the nature of oxymuriatic acid, must be considered as a compound of muriatic acid and phosphorus acid. It occurred to me, that if the acids of phosphorous really existed in these combinations, it would not be difficult to obtain them, and thus to gain proofs of the existence of oxygen in oxymuriatic acid.

I made a considerable quantity of the solid compound of oxymuriatic acid and phosphorus by combustion, and saturated it with ammonia, by heating it in a proper receiver filled with ammoniacal gas, on which it acted with great energy, producing much heat; and they formed a white opake powder. Supposing that this substance was composed of the dry muriates and phosphates of ammonia; as muriate of ammonia is very volatile, and as ammonia is driven off from phosphoric acid, by a heat below redness, I conceived that, by igniting the product obtained, I should procure phosphoric acid; I therefore introduced some of the powder into a tube of green glass, and heated it to redness, out of the contact of air, by a spirit lamp; but found to my great surprise, that it was not at all volatile nor decomposable at this degree of heat, and that it gave off no gaseous matter.

The circumstance, that a substance composed principally of oxymuriatic acid and ammonia should resist decomposition or change at so high a temperature, induced me to pay particular attention to the properties of this new body.

It had no taste nor smell; it did not seem to be soluble, nor did it undergo any perceptible change when digested in boiling water: it did not appear to be acted upon by sulphuric, muriatic, or nitric acids, nor by a strong lixivium of potash. The only processes by which it seemed susceptible of decomposition were by combustion, or the action of ignited hydrat of potash. When brought into the flame of a spirit lamp and made red-hot, it gave feeble indications of inflammation, and tinged the flame of a yellow color, and left a fixed acid having the properties of phosphoric acid. When acted on by red-hot hydrat of potash, it emitted a smell of ammonia, burnt where it was in contact with air, and appeared to dissolve in the alkali. The potash which had been so acted upon gave muriatic acid, by the addition of sulphuric acid.

I heated some of the powder to whiteness, in a tube of platina; but it did not appear to alter; and after ignition gave ammonia by the action of fused hydrat of potash.

I caused ammonia, made as dry as possible, to act on the phosphuretted liquor of M. M. Gay Lussac and Thenard; and on the sulphuretted muriatic liquor of Dr. Thomson; but no decomposition took place; nor was any muriate of ammonia formed when proper precautions were taken to exclude moisture. The results were new combinations; that from the phosphuretted liquor was a white solid, from which a part of the phosphorus was separated by heat; but which seemed no further decomposable, even bv ignition. That from the sulphuretted liquor was likewise solid, and had various shades of color, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia; but as these compounds did not present the same uniform and interesting properties as that from the phosphoric sublimate, I did not examine them minutely: I contented myself by ascertaining that no substance known to contain oxygen could be procured from oxymuriatic acid, in this mode of operation.

It has been said, and taken for granted by many chemists, that when oxymuriatic acid and ammonia act upon each other, water is formed; I have several times made the experiment, and I am convinced that this is not the case. When about 15 or 16 parts of oxymuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gases, and from five to six parts of nitrogen are produced; and the result is dry muriate of ammonia.

Mr. Cruikshank has shown that oxymuriatic acid and hydrogen, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water; and M. M. Gay Lussac and Thenard have stated that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oxymuriatic acid gas and hydrogen. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapour, and a condensation of from $\frac{1}{10}$ to $\frac{1}{20}$ of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oxymuriatic acid and the hydrogen by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures; but I have never been able to avoid a slight condensation; though, in proportion as the gases were free from oxygen or water, this condensation diminished.

I mixed together sulphuretted hydrogen in a high degree of purity and oxymuriatic acid gas, both dried, in equal volumes: in this instance the condensation was not $\frac{1}{40}$; sulphur, which seemed to contain a little oxymuriatic acid, was formed on the sides of the vessel; no vapour was deposited; and the residual gas contained about $\frac{19}{20}$ of muriatic acid gas, and the remainder was inflammable.

M. M. Gay Lussac and Thenard have proved by a copious collection of instances, that in the usual cases where oxygen is procured from oxymuriatic acid, water is always present, and muriatic acid gas is formed: now, as it is shown that oxymuriatic acid gas is converted into muriatic acid gas by combining with hydrogen, it is scarcely possible to avoid the conclusion, that the oxygen is derived from the decomposition of water, and, consequently, that the idea of the existence of water in muriatic acid gas is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxygen in oxymuriatic acid gas.

M. M. Gay Lussac and Thenard indeed have stated an experiment, which they consider as proving that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious that in this case they formed the same compound as that produced by the action of oxymuriatic acid on lead; and in this process the muriatic acid must lose its hydrogen, and the lead its oxygen; which of course would form water: these able chemists, indeed, from the conclusion of their memoir, seem aware that such an explanation may be given, for they say that the oxymuriatic acid *may be* considered as a simple body.

I have repeated those experiments which led me first to suspect the existence of combined water in muriatic acid, with considerable care; I find that, when mercury is made to act upon 1 in a volume of muriatic acid gas, by Voltaic electricity, all the acid disappears, calomel is formed, and about ·5 of hydrogen evolved.

With potassium, in experiments made over very dry mercury, the quantity of hydrogen is always from nine to eleven, the volume of the muriatic acid gas used being 20.

And in some experiments made very carefully by my brother Mr. John Davy, on the decomposition of muriatic acid gas, by heated tin and zinc, hydrogen equal to about half its volume was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oxymuriatic gas, resulted.

It is evident from this series of observations, that Scheele's view (though obscured by terms derived from a vague and unfounded general theory) of the nature of the oxymuriatic and muriatic acids may be considered as an expression of facts; whilst the view adopted by the French school of chemistry, and which, till it is minutely examined, appears so beautiful and satisfactory, rests, in the present state of our knowledge, upon hypothetical grounds.

When oxymuriatic acid is acted upon by nearly an equal volume of hydrogen, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted on by mercury, or any other metal, the oxymuriatic acid is attracted from the hydrogen, by the stronger affinity of the metal; and an oxymuriate, exactly similar to that formed by combustion, is produced.

The action of water upon those compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oxymuriatic acid with inflammable bases, may be easily explained, according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystalized mass is obtained, from which oxide of tin and muriate of ammonia can be procured by ammonia. In this case, oxygen may be conceived to be supplied to the tin, and hydrogen to the oxymuriatic acid.

The compound formed by burning phosphorus in oxymuriatic acid is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids; oxygen, it may be supposed, is furnished to the phosphorus to form phosphoric acid, hydrogen to the oxymuriatic acid to form common muriatic acid gas.

None of the combinations of the oxymuriatic acid with inflammable bodies can be decomposed by dry acids; and this seems to be the test which distinguishes the oxymuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash for instance, if M. Berthollet's estimation of its composition approaches towards accuracy, when ignited, is a compound of oxymuriatic acid with pottassium: muriate of ammonia is a compound of muriatic acid gas and ammonia; and when acted on by potassium, is decompounded: the oxymuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hydrogen are set free.

The vivid combustion of bodies in oxymuriatic acid gas, at first view, appears a reason why oxygen should be admitted in it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids, become ignited during their mutual agency; and such an effect might be expected in an operation so rapid, as that of oxymuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oxymuriatic acid consists of an acid basis united to oxygen, exists in the general analogy of the compounds of oxymuriatic acid, and metals, to the common neutral salts: but this analogy, when strictly investigated, will be found to be very indistinct; and even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water, and the metals of bases with hydrogen; and that, in the case of the action of oxymuriatic acid and metals, metal furnishes hydrogen to form muriatic acid, and a basis to produce the neutral combination.

That the quantity of hydrogen evolved during the decomposition of muriatic acid gas by metals, is the same that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hydrogen with oxymuriatic acid, one quantity must always be separated. Hydrogen is disengaged from its oxymuriatic combination, by a metal, in the same manner as one metal is disengaged by another from similar combinations, and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hydrogen is that which seems to adhere to oxymuriatic acid with the least force.

I have caused strong explosions from an electrical jar to pass through oxymuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change.

I electrized the oxymuriates of phosphorus and sulphur for some hours, by the power of the Voltaic apparatus of 1000 double plates: no gas separated, but a minute quantity of hydrogen, which I am inclined to attribute to the presence of moisture in the apparatus employed; for I once obtained hydrogen from Libavius's liquor by a similar operation; but I have ascertained that this was owing to the decomposition of water adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled, there was no production of any permanent elastic matter.

As there are no experimental evidences of the existence of oxygen in oxymuriatic acid gas, a natural question arises concerning the nature of these compounds, in which the muriatic acid has been supposed to exist, combined with much more oxygen than oxymuriatic acid, in the state in which it has been named, by Mr. Chenevix, hyperoxygenized muriatic acid.

Can the oxymuriatic *acid* combine either with oxygen or hydrogen, and form with each of them an acid compound; of which that with hydrogen has the strongest, and that with oxygen the weakest affinity for bases? for the able chemist to whom I have just referred, conceives that hyperoxymuriates are decomposed by muriatic acid. Or, is hyperoxymuriatic acid, the basis of all this class of bodies, the most simple form of this species of matter?

The phenomena of the composition and docomposition of the hyperoxymuriates may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyperoxygenized, from hyperoxymuriate of potash by various modes, but uniformly without success. By distilling the salt with dry boracic acid, though a litle oxymuriatic acid is generated, yet oxygen is the chief gaseous product, and a muriate of potash not decomposable is produced.

The distillation of the orange-coloured fluid, produced by dissolving hyperoxymuriate of potash in sulphuric acid, affords only oxygen in great excess, and oxymuriatic acid.

When solutions of muriates or muriatic acid are electrized in the Voltaic circuit, oxymuriatic acid is evolved at the positive surface, and hydrogen at the negative surface. When a solution of oxymuriatic acid in water is electrized oxymuriatic acid and oxygen appear* at the positive surface, and hydrogen at the negative surface; facts which are certainly unfavourable to the idea of the existence of hyperoxygenized muriatic acid, whether it be imagined a compound of oxymuriatic acid with oxygen, or the basis of oxymuriatic acid.

If the facts respecting the hyperoxymuriate of potash, indeed be closely reasoned upon, it must be regarded as nothing more than as a triple compound of oxymuriatic acid, potassium and oxygen. We have no right to assume the existence of any peculiar acid in it, or of a considerable portion of combined water; and it is perhaps more conformable to the analogy of chemistry, to suppose the large quantity of oxygen combined with the potassium, which we know has an intense affinity for oxygen, and which, from some experiments, I am inclined to believe, is capable of combining directly with more oxygen than exists in potash, than with the oxymuriatic acid, which as far as is known, has no affinity for that substance.

---

* The quantity of oxymuriatic acid in the aqueous solution is so small that the principal products must be referred to the decomposition of water. This happens in other instances; the water only is decomposed in dilute solutions of nitric and sulphuric acids.

It is generally supposed that a mixture of oxymuriatic acid and hyperoxymuriatic acid is disengaged when hyperoxymuriate of potash is decomposed by common muriatic acid* but I am satisfied from several trials, that the gas produced in this way, when not mixed with oxygen, unites to the same quantity of hydrogen,† as common oxymuriatic acid gas from manganese; and I find, by a careful examination, that the gas disengaged during the solution of platina, in a mixture of nitric and muriatic acids, which has been regarded as hyperoxymuriatic acid, but which I stated some years ago to possess the properties of oxymuriatic gas,* is actually that body owing its peculiar colour to a small quantity of nitromuriatic vapor suspended in it, and from which it is easily freed by washing.

---

* If hyperoxymuriate of potash be decomposed by nitric or sulphuric acid, it affords oxymuriatic acid and oxygen If it be acted upon by muriatic acid, it affords a large quantity of oxymuriatic acid gas only. In this last case, the phenomenon seems merely to depend upon the decomposition of the muriatic acid gas, by the oxygen loosely combined in the salt.

† This likewise appears from Mr. Cruikshank's experiments. See Nicholson's Journal, vol. v. 4to, p. 206.

‡ The platina, I find by several experiments made with great care, has no share in producing the evolution of this gas. It is formed during the production of aqua regia. The hydrogen of the muriatic acid attracts oxygen from the nitric acid. Oxymuriatic acid gas is set free, and nitrous gas remains in the solution, and gives it a deep red colour. *Nitrous* acid and muriatic acid produce no oxymuriatic acid gas. Platina, during its solution is perfectly formed aqua regia, gives only nitrous gas and nitrous vapor; and I find, that rather more oxymuriatic acid gas is produced, by heating together equal quantities of nitric acid of 1·45, and muriatic acid 1·18, when they are not in contact with platina, than when exposed to that metal. The oxymuriatic acid gas produced from muriatic acid, by nitric acid, I find combines with about an equal volume of hydrogen by detonation.

Few substances, perhaps, have less claim to be considered as acid, than oxymuriatic acid. As yet we have no right to say that it has been decompounded; and as its tendency of combination is with pure inflammable matters, it may possibly belong to the same class of bodies as oxygen.

May it not in fact be a *peculiar* acidifying and dissolving principle, forming compounds with combustible bodies, analogous to acids containing oxygen or oxides, in their properties and powers of combination; but differing from them, in being for the most part decomposable by water. On this idea muriatic acid may be considered as having hydrogen for its basis, and oxymuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oxymuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oxymuriatic acid, may be regarded as analogous bodies, The combinations of oxymuriatic acid with lead, silver, mercury, potassium and sodium, in this view would be considered as a class of bodies related more to oxides than acids, in their powers of attraction.

It is needless to take up the time of this learned society by dwelling upon the imperfection of the modern nomenclature of these substances. It is in many cases connected with false ideas of their nature and composition; and in a more advanced state of the inquiry, it will be necessary for the progress of science, that it should undergo material alterations.

It is extremely probable that there are many combinations of the oxymuriatic acid with inflammable bodies which have not been yet investigated. With phosphorus it seems capable of combining in at least three proportions; the phosphuretted muriatic acid of Gay Lussac and Thenard is the compound containing the maximum of phosphorus. The crystalline phosphoric sublimate, and the liquor formed by the combustion of phosphorus in oxymuriatic acid gas, disengage no phosphorus by the action of water; the sublimate, as I have already mentioned, affords phosphoric and muriatic acid: and the liquid I believe only phosphorous acid and muriatic acid.

The sublimate from the boracic basis gives, I believe, only boracic and muriatic acid, and may be regarded as boracium acidified by oxymuriatic acid.

It is evident, that whenever an oxymuriatic combination is decomposed by water, the oxide or acid or alkali or oxidated body formed must be in the same proportion as the muriatic acid gas, as the oxygen and hydrogen must bear the same relation to each other; and experiments upon these compounds will probably afford simple modes of ascertaining the proportions of the elements, in the different oxides, acids, and alkaline earths.

If, according to the ingenious idea of Mr. Dalton, hydrogen be considered as one in weight, in the proportion it exists in water, then oxygen will be nearly 7·5; and assuming that potash is composed of one proportion of oxygen, and one of potassium, then potash will be 48, and potassium* about 40-5; and from an experiment which I have detailed in the last Bakerian lecture, on the combustion of potassium in muriatic acid gas, oxymuriatic acid will be represented by 32·9, and muriatic acid gas of course by 33·9; and this estimation agrees with the specific gravity of oxymuriatic acid gas, and muriatic acid gas. From my experiments, 100 cubical inches of oxymuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74·5 grains; whereas by estimation they should weigh 74·6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38·4 grains.

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide or of acid it would furnish by the action of water, and consequently the quantity of oxygen with which the inflammable matter will combine.†

---

* Supposing potash to contain nearly 15·6 per cent of oxygen.

† I have stated in the last Bakerian lecture, that during the decomposition of the amalgam from ammonia, one in volume of

In considering the dry muriates, as compounds of oxymuriatic acid and inflammable bodies; the argument that I have used in the last Bakerian lecture, to show that potassium does not form hydrate of potash by combustion, is considerably strengthened; for from the quantity of oxymuriatic acid the metal requires to produce a muriate, it seems to be shown that it is the simplest known form of the alkaline matter. This I think approaches to an experimentum crucis. Potash made by alcohol, and that has been heated to redness, appears to be a hydrate of potash, whilst the potash formed by the combustion of potassium must be considered as a pure metallic oxide, which requires about 19 per cent. of water to convert into a hydrate.

Amongst all the known combustible bodies, charcoal is the only one which does not combine directly with oxymuriatic acid gas, and yet there is reason for believing that this com-

---

hydrogen to two of ammonia is evolved: it is remarkable that whatever theory of the nature of this extraordinary compound be adopted there will be a happy coincidence as to definite proportions. If it be supposed that the hydrogen arises from the decomposition of water; then the oxygen that must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hydrogen, in an equal volume of muriatic acid; or if it be said that ammonium is a compound of two of ammonia and one of hydrogen in volume, then equal volumes of muriatic gas and ammonia will produce the same compound as oxymuriatic acid and ammonium, supposing they could be immediately combined. I once thought that the phenomena of metallization might be explained according to the modified phlogistic theory, by supposing three different classes of metallic bodies: First, the metal of ammonia, in which hydrogen was so loosely combined as to be separable with great ease, and which, in consequence of the small affinity of the basis for water, it had little tendency to combine with oxygen. The second, the metals of the alkalies and alkaline earths, in which the hydrogen was more firmly combined, but in combustion, forming water capable of being separated from the basis. And, thirdly the metals of the earths and common metals, in which the hydrogen was more intimately combined; producing by union with oxygen, water not separable by any new attractions. The phenomena of the action of potassium and sodium upon muriatic acid, referred to in the text, seem however to overturn these speculations so far as they concern the metals from the fixed alkalies.

bination may be formed by the intermedium of hydrogen. I am inclined to consider the only substance produced by the action of oxymuriatic acid gas, and olefiant gas, as a ternary compound of these bodies; for they combine nearly in equal volumes; and I find that by the action of potassium upon the oil so produced, muriate of potash is formed, and gaseous matter, which I have not yet been able to collect in sufficient quantity to decide upon its nature, is formed. Artificial camphor, and muriatic ether, as is probable from the ingenious experiments of M. Gehlen and M. Thenard, must be combinations of a similar kind, one probably with more hydrogen, and the other with more carbon.

One of the greatest problems in economical chemistry, is the decomposition of the muriates of soda and potash. The solution of this problem will, perhaps, be facilitated by these new views. The affinity of potassium and sodium for oxymuriatic acid is very strong; but so likewise is their attraction for oxygen, and the affinity of their oxides for water. The affinities of oxymuriatic acid gas for hydrogen, and of muriatic acid gas for water, are likewise of a powerful kind. Water, therefore, should be present in all cases, when it is intended to attempt to produce alkali. It is not difficult after these views to explain the decomposition of common salt, by aluminous or silicious substances, which, as it has been long known, act only when they contain water. In these cases the sodium may be conceived to combine with the oxygen of the water and with the earth, to form a vitreous compound; and the oxymuriatic acid to unite with the hydrogen of the water, forming muriatic acid gas.

It is also easy, according to these new ideas, to explain the decomposition of salt by moistened litharge, the theory of which has so much perplexed the most acute chemists. It may be conceived to be an instance of compound affinity: the oxymuriatic acid is attracted by the lead, and the sodium combines with the oxygen of the litharge and with water to form hydrate of soda, which gradually attracts carbonic acid from the air.

As iron has a strong affinity for oxymuriatic acid, I attempted to procure soda by passing steam over a mixture of iron filings, and muriate of soda intensely heated; and in this way I succeeded in decomposing some of the salt; hydrogen came over: a little hydrate of soda was formed, and muriate of iron was produced.

It does not seem improbable, supposing the views that have been developed accurate, that by complex affinities, even potassium and sodium in their metallic forms may be produced from their oxymuriatic combinations; for this purpose the oxymuriatic acid should be attracted by one substance, and the alkaline metals by another; and such bodies should be selected for the experiment, as would produce compounds differing considerably in degree of volatility.

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the society to some of the theorectical relations of the facts noticed in the preceding pages.

That a body principally composed of oxymuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phenomenon of a perfectly new kind. Three bodies, two of which are permanent gases, and the other of which is considerably volatile, form in this instance a substance neither fusible nor volatile, at a white heat. It could not have been expected that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oxymuriatic acid, would have appeared incredible, according to all the existing analogies of chemistry. The experiments on which these conclusions are founded, are, however, uniform in their results: and it is easy to repeat them. They seem to show, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oxymuriatic acid, phosphorus and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common reagents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary, may be reduced into simpler forms of matter? And that an intense attraction, and an equilibrium of attraction, may give to a compound, containing several constituents, that refractory character, which is generally attributed to unity of constitution, or to the homogeneous nature of its parts?

Besides the compound of the phosphoric sublimate and ammonia, and the other analogous compounds which have been referred to, it is probable that other compounds of like nature may be formed of the oxides, alkalies, and earths, with the oxymuriatic combinations, or of the oxymuriatic compounds with each other; and should this be the case, the more refined analogies of chemical philosophy will be extended by these new, and, as it would seem at first view, contradictory facts. For, if, as I have said, oxymuriatic acid gas be referred to the same class of bodies as oxygen gas, then, as oxygen is not an acid, but forms acids by combining with certain inflammable bodies, so oxymariatic acid, by uniting to similar substances, may be conceived to form either acids, which is the case when it combines with hydrogen, or compounds like acids or oxides, capable of forming neutral combinations, as in the instances of the oxymuriates of phosphorus and tin.

Like oxygen, oxymuriatic acid is attracted by the positive surface in Voltaic combinations, and on the hypothesis of the connexion of chemical attraction with electrical powers, all its energies of combination correspond with those of a body supposed to be negative in a high degree.

And in most of its compounds, except those containing the alkaline metals, which may be conceived in the highest degree positive, and the metals with which it forms insoluble compounds, it seems still to retain its negative character.

I shall occupy the time of the society for a few minutes longer only, for the purpose of detailing a few observations connected with the Bakerian lectures, delivered in the two last years; particularly those parts of them relating to sulphur and phosphorus, which new and more minute inquiries have enabled me to correct or extend.

I have already mentioned that there are considerable differences in the results of experiments, made on the action of potassium, on sulphur and phosphorus, and their combinations with hydrogen according to different circumstances of the process. I shall now refer to such of these circumstances as I have been able fully to investigate.

The able researches of Dr. Thomson, have shown that sulphur, in its usual state, contains small quantities of acid matter; and though in my first experiments, I conceived that by employing crystallized native sulphur, which had been recently sublimed in nitrogen, I should avoid the presence of any foreign matter, yet I am inclined to believe that this is not the case; for by subliming some similar sulphur in nitrogen, I find that litmus paper placed in the upper part of the retort is slightly reddened.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid; sulphuretted hydrogen likewise is soluble in muriatic acid: and this circumstance led me to underrate the quantity of sulphuretted hydrogen given off in experiments of this kind.*

---

* This circumstance has been pointed out by M. M. Gay Lussac and Thenard, in a paper printed in the Journal de Physique for December, in which these gentlemen endeavour to show that,

In acting upon sulphuretted hydrogen by potassium in my early experiments, I used large quantities of the gas and of the metal ; and in these cases I have reason to believe that the violence of the combustion occasioned the decomposition of a considerable quantity of the gas ; and, in consequence, led me to form erroneous conclusions concerning the nature of this curious operation.

In all late experiments in which sulphur or sulphuretted hydrogen was concerned, I have used muriatic acid saturated with sulphuretted hydrogen over mercury. I have employed sulpur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green glass or plate glass lined with sulphur and filled with very pure nitrogen or hydrogen. In making potassium act upon sulphuretted hydrogen, I have employed the gas only in quantities of from one to three cubical inches, and have made the combination in narrow curved tubes of green glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is a sufficient correspondence between them to enable me to form conclusions, which I may venture to say cannot be far from the truth.

When one grain of potassium, which would give by the action of water about one cubical inch and $\frac{1}{16}$ of hydrogen is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light, and from $\frac{1}{14}$ to $\frac{1}{16}$ of a cubical inch of sulphuretted hydrogen is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hydrogen, affords from $\frac{9}{10}$ to $\frac{11}{10}$ of a cubical inch of pure sulphuretted hydrogen.

---

whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it produces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same Journal in which their memoir has appeared) offered some observations on their inquiries.

When more sulphur is used so as to be from twice to ten times the weight of the potassium, the quantity of sulphuretted hydrogen evolved by the action of the acid, is from $\frac{7}{10}$ to $\frac{9}{10}$ but if heat be applied to the combination, so as to drive off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used ; and I am inclined to believe, from the phenomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one prportion, in which the metal is to the sulphur nearly as three to one in weight ; and in which the quantities are such that the compound burns into neutral sulphate of potash.

When a grain of potassium is made to act upon about 1·1 cubical inches of sulphuretted hyrodgen, all the hydrogen is set free, and a sulpuret of potassium containing one fourth of sulphur is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

When sulphuretted hydrogen is employed in large quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hydrogen disengaged, and a compound of sulphuretted hydrogen and sulphuret of potash is formed, which gives sulphuretted hydrogen by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

From a number of experiments I am inclined to believe that potassium and phophorous, in whatever quantities they are heated together combine only in one proportion, a grain of potassium requiring about one third of a grain of phosphorus to form a phosphuret ; which when acted upon by muriatic acid, produces from $\frac{8}{10}$ to $\frac{10}{10}$ of a cubical inch of phosphuretted hydrogen.

Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hydrogen, and sets free rather more than four cubical inches of hydrogen, and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

If, according to Mr. Dalton's ideas of proportion, the quantity in which sulphur enters into its combination were to be deduced from its union with potassium, in which it seems to form about one forth the weight of the compound, the number representing it would be 13·5. I have lately weighed sulphuretted hydrogen and sulphurous acid gas, with great care: the specific gravity of the first at mean temperature and pressure, from my experiments, is 10645, which differs very little from the estimation of Mr. Kirwan, that of sulphurous acid gas I find is 20967. Sulphuretted hydrogen, as I have shown, contains an equal volume of hydrogen; and on this datum the number representing sulphur is 13·4. I have never been able to burn sulphur in oxygen without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphurous acid from 100 of oxygen in volume; from which I am inclined to believe, that sulphurous acid consists of sulphur dissolved in an equal volume of oxygen; which would give the number as 13·7* nearly considering the acid gas as containing one proportion of sulphur, and two of oxygen; and these estimations do not differ from each other materially.

I have made several experiments on the combustion of phosphorus in oxygen gas. From the most accurate, I am inclined to conclude that 25 of phosphorus absorbed in combustion about 34 of oxygen in weight: and considering phosphoric acid as compound of three portions of oxygen and one of phosphorus, the number representing phosphorus, will be about 16·5, which is not very remote from the number that may be deduced from the composition of phosphuret of potassium.

---

* The estimation from the composition of sulphuretted hydrogen, must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian lecture, I have estimated the specific gravity of sulphuretted hybrogen at 35 grans the 100 cubical inches, which was not far from the mean between the estimations of Mr. Kirwan and Mr. Thenard. According to this last experiment, sulphuretted hydrogen is composed of one portion of hydrogen, represented by 1, and one of sulphur represented by 13·4.

The numbers which represent the proportions in which sulphur and phosphorus unite with other bodies, are such as do not exclude the existence of combined portions of oxygen and hydrogen in their constitution; but it may be questioned, whether the opinion which I formed, that the inflammable gas disengaged from them by electricity, is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian lecture is capable of forming a solid hydruret; and a part of the sulphur distilled from iron pyrites is usually of a soft consistence, and emits the smell of sulphuretted hydrogen, and probably contains that body. It is not unlikely, that in all cases, phosphorus and sulphur contain small quantities of the hydrurets of phosphorus and sulphur; and the producduction of a minute portion of sulphuric acid in the slow combustion of sulphur, is probably connected with the production of water. Though the pure oxides of sulphur and phosphorus have never been obtained, yet from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe that they sometimes exist in minute quantities in common phosphorus and sulphur, and with hydrogen give to them their variable properties.

The colours of different specimens of phosphorus, as well as of sulphur, differ considerably; the red colour of phosphorus as it is commonly prepared, is probably owing to a slight mixture of oxide. Common roll sulphur is of a very pale yellow, the Sicilian sulphur of an orange colour, and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish-green colour. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxygen in Sicilian sulphur, which may give rise to sulphuric acid in distillation, or to sulphuric acid itself.

Conceiving that, if definite proportions of oxgen and hydrogen exist in sulphur and phosphorus, they ought to be manifested in the agency of oxymuriatic acid gas on these bodies, I made some experiments on the results of these operations. In the first trial, on the combination of sulphur with oxymuriatic acid gas, I employed five grains of roll sulphur, and admitted the gas into the exhausted retort, from a vessel in which it had been in contact with warm water: in this case more than half a cubical inch of oxygen gas, and nearly two cubical inches of muriatic acid gas, were produced. Suspecting in this instance, that aqueous vapor had been decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime: in this case, though Sicilian sulphur was used, no oxygen gas was evolved; and not half a cubical inch of muriatic acid: the quantity was the same as in the last experiment; and it was found, that between 16 and 17 cubical inches of oxymuriatic acid gas disappearde; the whole of the sulphur was sublimed in the gas, and the liquor formed was of a tawny orange colour.

No oxygen was expelled during the combustion of phosphorus in oxymuriatic acid gas, nor could I ascertain that any muriatic acid had been formed; three grains of phosphorus were entirely converted into sublimate, by the absorption of about 23 cubical inches and a half of the gas.

It would seem from these quantities, that the sulphuretted liquor formed by subliming sulphur in oxymuriatic acid gas, consists of one proportion of sulphur, represented by 13·5, and one of oxymuriatic gas, represented by 32·9, and that the phosphoric sublimate must be composed of three portions of oxymuriatic gas, represented by 98·7 and one of phosphorus respresented by 16·5

Harsnepe's Case of Canine Madness.

Reister's-Town, Baltimore County, May 27, 1811.

DEAR SIR,

Agreeably to your request, I herewith transmit the statement of a case of hydrophobia, which was treated successfully.

Negro Matilda, aged eleven years, belonging to Mr. Nathaniel Watts, about seven miles from this village, was bit by a mad dog, on the evening of the seventh of April, 1810. As the part affected appeared to be only a slight scratch, it was not thought of sufficient importance to require medical assistance; the result however proved the reverse. On the eighth day* after she receivedthe injury, about ten o'clock in

---

* When the encounter with the dog took place, the girl was alone, and from home. In detailing the particulars soon after her return, she remarked "*that the dog was very weak*, because she easily beat him off with a stick, and he then scrambled over a fence with great difficulty." He was seen the next morning not quite a mile from the place where the girl left him, but never afterwards, although there was a diligent search for him by some of the neighbours. It was fair to conclude that he died very soon after the girl had been bit; otherwise he would have been discovered in a country so populous, and where so great an alarm had been excited.

The short space that elapsed from the time the virus was inflicted, till the symptoms of canine madness were apparent, seems to lead to a conclusion corroborated by many other facts, viz. that the virus of rabid animals is virulent in proportion to the continuance of their disease. Among many other arguments in support of this opinion, I state the following facts which came within my own knowledge, but were recorded by my father. "A dog belonging to a near neighbour, bit three cows, seven hogs and two dogs, before it was ascertained that he was mad. The whole twelve animals died mad; but the disease appeared at a period when it was not expected. The first case was one of the dogs, six months after he had been bit, but none of the others were affected till the ninth month, and before the end of the tenth, they were all mad. The dog that bit them, was discovered to be indisposed eight or ten hours before they were bit, and certainly the morning she was sent to a spring for a bucket of water, which she carried to the house as usual, and returned for a second. The spring is about three feet in diameter at its surface, and when she came very near it she threw her bucket down, and ran precipitately into an adjoining wood. Mr. Watts' son observed her, saw her throw away the bucket, and repeatedly called to her to stop, but she paid no attention to his calls. He followed her, and as soon as she perceived that he was likely to overtake her, she turned towards him, apparently with an intention of seizing him. He retreated, she pursued, and he made his escape by geting a fence between them. As she ran through the wood she foamed copiously at the mouth, and the saliva ran down her jaws. She now proceeded in a brisk trot (which those who saw her compared to that of a dog) towards the house, and soon became more composed. By this time the family had assembled and collected some of the near neighbours, by whose united exertions she was driven into an out house. Before she went into the house one of Mr. Watts' sons approached her and she attempted to seize him.

The features of the disease were now so manifest, that it was evident death must soon be the consequence, unless medical aid could be speedily procured. A messenger was therefore immediately dispatched for me. Being a considerable distance from home, it was not in my power to reach her before eight o'clock in the evening.—I found her exceedingly depressed in both body and mind, speechless, and apparently senseless, sitting on some straw on the floor, with her body bent forwards, and her arms and legs closely contracted to it. Her eyes were fixed, the balls highly inflamed and greatly

no appearance of disease was discernible twenty four hours before. The same year (1785) a mad dog that had been harrassed for two or three weeks, bit a cow, three dogs, two horses and several hogs, on the farm of John Russel, all of which were seized with madness within two weeks, one of the dogs was attacked in five days, and one of the horses on the seventh. This dog exhausted by his pursuers and the disease, died the same night he did the mischief at Russell's house." [*Editor.*

protruded, the lids widely distended, which altogether gave her a wild maniacal yet melancholy appearance. Her pulse was natural, except that it was fuller than in health. Her extremities were stiff, and great force was required to extend them; they might aptly have been compared to a piece of lead that had been bent, for they remained in whatsoever position they were put, notwithstanding great force was required to bend them. Upon examining the bitten part I found it somewhat swollen and considerably inflamed; but no discharge was to be discovered, nor had there been any before.

I drew twelve ounces of blood from the arm, and immediately prepared the following bolus.

R. G. Camphor, Grs. xviij.
Tr. Opi. Gttæ: xviij.
Calomel Grs. iv.
Cons: Ros: Q. S. Fiat: Bol: 1.

The medicine was mixed with a little tea, and on attempting to pour it down her throat (the action of the muscles opposing) a large quantity of saliva was dischaged of a consistence so tenacious that it had an appearance almost membranous. By these efforts a great part of the medicine was lost. After three attempts to give her a bolus, I do not think she swallowed more than the contents of one. She swallowed all fluids with great difficulty; but she was little distressed in swallowing solids.

An epispastic was now applied over the bitten part, and I ordered a quantity (how much I do not now recollect) of the Ung: Mercur: Fort: to be rubbed on her thighs, and under her throat. About forty minutes after she had taken the bolus, the rigidity of her limbs relaxed, and it would appear that the organs of speech partook of the same effects, for she spoke and gave intelligent answers. I asked her how she was? She answered, very well. I asked her whether she felt pain? She replied, not then, but that she had felt a violent pain in her arm, in the morning. I enquired of her in which arm she had felt the pain? In this she said, putting her other hand on the bitten part. I requested her to inform me how it had effected her? It ran, said she, from this place, (putting her hand again on the part bitten) up my arm and all over my body. I remained at the house all night, and repeated the bolus eight hours after she had taken the first. It was with great difficulty I could get it down, for the reasons already assigned. She again discharged a large portion of the same sort of tough viscid saliva. She sat up a great part of the night, though she sometimes laid down, and occasionally got a little sleep. Several times during the night, she threw out of her mouth considerable quantities of tough, mucilaginous saliva, seemingly by the joint action of the muscles of deglutition, and a partial effort of the stomach to evacuate itself. The next morning I gave her another bolus, which she swallowed with great difficulty, and discharged saliva in quantity and quality as before. I found that the best mode of administering the bolus was to give some warm drink previously, to produce a discharge of the saliva, and to follow it immediately by the medicine.—At this time, I noticed the pulse particularly, and found it very variable. At one time it was very weak and low, and in ten or fifteen minutes, it would become perfectly natural: in the same time again it would become very full, approaching to hardness. This variable state of the pulse continued several days. As the patient was now sensible, I gave her a small portion of calomel and desired her to rub it on her gums and internal fauces, which she did according to my directions. This practice was pursued morning and evening till the mouth was affected. I did not direct the use of the calomel in this way, with a view of salivating, but under the impression that it would act as a specific against the virus. The calomel did not produce purgative effects, for the intestinal canal was unsually torpid; so much so, that injections frequently became necessary, and appeared to be useful.—The secretions did not appear to be materially altered, except that of the saliva—the urine was no way changed as to quantity or quality.—The perspiration was a little increased on the two or three first days of the disease, the skin being soft during that period. The tongue exhibited a natural appearance, except a small red streak in the middle through its whole length, which was of a scarlet red, and perfectly dry. This appearance vanished at the commencement of the ptyalysm, for after that took place, the case became altogether a mercurial disease, and was managed as such.

I left my patient at ten o'clock in the morning, (second day of the disease) and returned in the evening, when I was informed that she was worse. She had been talking incoherently, grated her teeth frequently, and snapped them together with great violence. She expressed a desire to see the dog, and declared that she would bite him. A vast concourse of people assembled to see a sight so novel and interesting. She took but little notice of them, except one lady. During the violent spasmodic struggles with which she was tormented, she insisted that this woman was the dog, and used every possible exertion to bite her. The lady wore a black gown, and it was remarkable that the dog was black. She seemed to have associated the idea of the person and the dog by colour alone. The boluses were continued every eight hours, till Tuesday, the seventeenth; she then took half the quantity every four hours, till Thursday the nineteenth, when observing a ptyalysm to have commenced, the medicine was administered less frequently, once, twice or thrice in twenty-four hours, and sometimes in forty-eight hours. During the internal use of the medicine, the mercurial friction had been used as already directed, each night and morning, but was now applied less fqrequently, and more sparingly. Although the ptyalysm was well established in six or seven days from the first prescription, I increased the action of the mercury gradually, and lest the untoward symptoms, of so horrible a malody should recur, it was kept up six weeks. The blistered parts was kept open by epispastic ointment. In the height of the salivation, an accidental exposure to cold and dampness augmented the effects of the mercury exceedingly, and she now became very ill of the mercurial disease. These appearances, more formidable in appearance than reality, soon began to subside, and she finally recovered. I have seen my old patient repeatedly since, and she enjoys perfect health. Perhaps I ought to have noticed more particularly, that the discharge of viscid saliva on swallowing liquids, the difficuly of swallowing fluids, together with the variable state of the pulse, vanished at the first dawn of the salivation.

These are the principal facts that occurred in the above case, and are offered to you for such use as you may think proper to make of them, by

Your obedient Servant,
WILLIAM HARSNEPE.

Nathaniel Potter, M. D.

……

WILLIAMSON

On the connection between Scrofula and Phthysis Pulmonalis.

(Continued.)

WHEN the scrofula of the lungs terminates in a complete phthysis, I believe this species of consumption much more difficult to cure than that from pneumonia.*

That the phthysis pulmonalis of whatever nature, is in a great majority of cases fatal, is a truth proved by innumerable facts. That this should be the case, is by no means wonderful, for, it cannot be cured unless the ulcers in the lungs can be healed; and every body knows that it is almost impossible to heal ulcers in the lungs. The reason of this difficulty is thus accounted for by Galen—" Neque enim citra tussim expurgari possunt, illisque tussientibus laceratur locus. Itaque quasi per mutuas operas malum illis inorbem redit; quar namque lacerata sunt, rursus phlegmonem excitant; secundo iterum oportet maturari phlegmonem, etpus expurgari.* These objections will hold good in ulcers of the lungs from any cause. Ulcers of scrofulous tumours are difficult to cure when seated on the exterior of the body, they are no doubt much more so when they have their seat in the lungs.

---

* "Now although a vomica of the lungs is never without danger, yet experience teaches, that many more persons recover, when, after a suppuration in consequence of a pleurisy or a peripneumonia, they spit a large quantity of pus at once, than when the pus is collected in small tubercles in the lungs." Doctor Mead affirms the same. "This disease, although it be dangerous and often ends in consumption, yet is not so dangerous as those smaller ulcers."
Vanswieten Comment.

What Celsus said of the peripneumonia, may with much propriety be said of the phthysis pulmonalis, especially that of a scrofulous nature. " Id genus morbi plus periculi quam doloris habet."

When the patient is much emaciated, it may be necessary to have access to other strengthening medicines than the muriat of barytes and lime. In this case some of the bitters may be found more salutary than any other medicines with which we are acquainted. Among the bitters I think the bark preferable, in many cases, if the stomach will retain it, and not be too morbidly affected with its use. I am aware that many physicians oppose the use of bark in all such affections of the lungs.

When treating of the influenza of 1807,* I noticed this subject in a cursory manner, and adduced some authors of great celebrity in vindication of my opinion. I then referred more particularly to a species of disease which would render these objections more plausible than any that could be started against my present subject.

---

* For these cannot be cleansed without a cough, and in the act of coughing, the parts affected arc lacerated; so that the disorder in a manner increases, or produces itself; for the parts lacerated, are again thrown into a state of inflammation, when the inflammation must come a second time to maturity, and the matter be again cleansed out, &c. *Method. Medendi, lib. 5. cap. 2.*

† See Medical Repository, Hexade 3d, vol. 1st.

The bark has long been recommended in scrofula; if it can be of any service in this disease, the lungs being its seat, need not exclude the use of it. Not only modern physicians of eminence recommend the use of the bark, but also those of a more ancient date. Morton in speaking of the cinchona, says, "It soon safely stops the hemoptœ when present, and prevents it when apprehended. He gives a number of cases, in which he used it with the most happy effect in testimony of his opinion. Brunner confesses he did not believe what Morton said of the efficacy of the bark in an hemoptœ, till he found the truth of it by experience; and particularly in a thin man, subject to a bleeding at the nose, and afterwards troubled with a spitting of blood; at first only in spring and autumn; but in time the return of the complaint grew so frequent, that he was scarce ever free, for a day, from spitting blood, and his body was beginning to waste. Various remedies, and among the rest goats milk, having been used without success, the bark was given, after which the hemoptœ stopt in a few days and the man recovered his health." Brunner found the bark equally successful in a bleeding at the nose.

Torti used the bark in a number of consumptive cases, some of which were in the last stage of the disease before he used it; and although the disease was in these cases too far advanced to be cured, he nevertheless always found it to produce sensible relief for some days, and sometimes, though not always, with a manifest interruption of the accustomed exacerbation. He says, however, that in the case of a boy to whom he gave the bark at the urgent request of his father (expecting no good from it himself) it made a complete cure. He considered the boy consumptive, and he was generally supposed so; besides, he was afflicted with a spina ventosa. He observes that he afterwards saw him walking about the city so fleshy, strong and ruddy, that he scarcely knew him again, and quite free from all the symptoms of both consumption and spina ventosa. Torti relates other instances of consumptive patients being cured by the bark. Vanswieten says, " I have myself tried the bark in the beginning of consumption, and have not, repented of it. He adds, " I gave the bark under various forms, and for a long time, to a lady of very high birth, who by a vexatious cough and a slow fever, was emaciated without any hemoptœ having preceded: and although her strength was pretty much sunk, and she spat pus and had vicious conformation of the chest, yet she perfectly recovered."

Previous to using this medicine, Brunner and Torti had no faith in it, in these affections, and the latter gave it with much reluctance; therefore their testimony cannot be considered the child of blind prejudice, but of conviction, generated by unquestionable facts.

The justly distinguished Mead, gave the bark with advantage in hemoptysis, although he did not think it proper in confirmed consumption.

I shall conclude these remarks on the use of the bark in these diseases of the lungs, with the following extract from my history of the diseases of Queen Ann's county, Eastern Shore of Maryland, as contained in the second volume of Coxe's Medical Museum.

" During the spring season, catarrhal affections were frequent, and generally pretty easily cured by such remedies as are usually applied in that disease. The only obstinate catarrhal affection that I met with during my practice here, was that of a gentleman of an impaired constitution, who had long since been troubled with phthisical affections. He tried a great number of the medicines so generally prescribed in this disease, without the wished for effect. The disease still increasing, he became much debilitated and almost exhausted; his cough was very severe; every precaution was taken to prevent its return, and palliatives were given to shorten its duration, but in vain. In order to support the system, the bark was prescribed in as large quantities as his stomach would bear: this medicine had a far more happy effect than I anticipated; it not only supported the system, but also cured my patient.

Should the reader object to the authorities quoted as too antiquated, I would merely reply, that however antiquated they may be, some of them ranked amongst the first physicians of their age, and are even at this day looked up to with medical veneration. And we appear to be as unsuccessful, if not more so, in the treatment of these diseases, in this enlightened age, as the faculty were in ages that are past. This want of success cannot be owing to our better knowledge of them, nor to our treating them more judiciously.

The necessity of distinguishing between the different species of disease, and of studying the constitution of the patient, is obvious, for in cases of hemoptœ, if the patient be of a full or plethoric habit, the bark which has been and no doubt now is, so salutary in emaciated constitutions, would be very pernicious. These remarks are equally applicable to the consumption; and even in the scrofulous species we should not be too precipitate in its use.

In scrofulous affections of the lungs, the diet must not be so generous, as in other diseases of that nature, yet, that abstemious regimen, which is so salutary in a consumption from pneumonia, &c. is certainly very deleterious in this species of phthysis. In this species of the disease, the regimen should be nourishing but not heating.

In treating this disease we should endeavour to increase the tone of the general system, but sedulously avoid increasing the arterial action. At first, this may seem paradoxical; but we know that in diseases of great debility, the pulse is often more frequent, indeed always so in the phthysis, than in health, and that the tone of the general system may be increased without increasing the frequency of the pulse. Indeed as the system gains strength, the pulse becomes less frequent and more regular.

We should not only guard against a too stimulating diet, and one of a difficult digestion, but should also take care that too much is not taken at a time. If the stomach is surcharged, it will be morbidly affected, and this affection will soon be communicated through the nervous influence, to every, even the most remote parts of the system; as is frequently the case, even in health, after a full meal. In this case the lungs and brain are prominent sufferers. When the stomach is much distended, it is supposed to lessen the cavity of the thorax, by compressing the diaphragm. It is very probable that when the stomach is thus distended, that if it does not compress the diaphragm, and thus lessen the cavity of the thorax, that, from its contiguity to this membrane, it irritates and excites coughing. In persons labouring under the phthysis, the lungs and diaphragm appear to be morbidly irritable, and the least irritating cause produces cough. This is a fact which must be well known to every physician of an extensive practice. After becoming acquainted with the nervous association and influence throughout the general system, this is not surprising; and still less so, when we trace the *par vagum*. However, there are other pernicious effects from taking too much diet at once. The heart, stomach and lungs are very intimately related, and have a material influence over each other; and when one of them is diseased the others participate. In this connection, the lungs appear to hold the most prominent station. When the lungs refuse or cease to perform their function, animation ceases; when they perform it imperfectly, animation is imperfect; if they never perform it, we can never with propriety be called animated beings; hence Brown contended that life is a forced state.

The lungs are excited into action by the atmosphere; the atmosphere is inhaled into the lungs, and whilst there, a chemical decomposition takes place; the azotic and carbonic acid gases being expired, as obnoxious to life, while the oxygen is absorbed by the blood. This decomposition and new combination is necessary to health. When a deviation from this general rule takes place, disease ensues. If there is too much oxygen in the air and the lungs are diseased, it irritates them and proves injurious; as has been proved by the pneumatic physicians. I once witnessed this fact in a paralytic patient, to whom my preceptor administered it. If the air is not sufficiently oxygenated, respiration is laborious and difficult: therefore this is too important a subject in phthysis not to have claimed more attention than it has. Too pure an air, or one too impure, are both pernicious; and while we guard against the one as too stimulating, we should avoid the other as not sufficiently so. But when the air is as congenial as possible to health, if the lungs are diseased, their action must also be diseased. And I suspect, that in the consumption, the chemical decomposition of the air in the lungs is less perfect than in health, and that less oxygen is absorbed by the blood. This conjecture is rendered probable from too circumstances. 1st. I suspect there is not that aptitude for oxygen in the lungs of consumptive people, as in those who enjoy good health, for what is generally termed a pure air excites a coughing; hence Beddoes suggested the propriety of consumptive patients living in cow houses. 2nd. Pus, discharged from consumptive lungs, indicate a want of tone in the parts.

Oxygen increases the tone of the general system, and should therefore increase that of the lungs. It is not only the tone of the vessels of the lungs that is impaired in consumption, but the solids in general are; and in this general loss of tone the stomach greatly partakes, hence indigestion, &c. is the consequence. This would be a sufficient reason, were we destitute of another, for guarding against improper diet or full meals in the disease in question.

When the tone of the stomach is impaired, if too much food is taken, or such as is difficult of digestion, a pain in the stomach succeeded by diarrha is the consequence, and each of these complaints are very hurtful to the general system.

(To be continued.)

N. B. The remarks above, relative to the decomposition of the air in the lungs of consumptive persons are conjectural. It may be as I suppose; it may be the reverse, or it may be that the lungs in this respect do not deviate from the general law of nature. Be it as it may, it is certainly an interesting subject, and one which I hoped to have fully investigated before this day, but have been prevented from pursuing the investigation by having partly declined the practice of medicine. Should the publishing of these conjectures, even should they be proved erroneous, be a means of exciting some votary of the healing art, to take up the subject and determine the point, I shall be gratified with the belief that I have done the cause of humanity some small service; and he who shall do it, will not only receive honour in this century, but in ages yet to come for the deed.

……

Sanford on the Peruvian Bark.

SIR,

THE importance of making any improvement in the preparation of so valuable a medicine as the Peruvian bark, is obviously so great, that if any hints herein stated should be deemed to promote that effect, I shall not doubt but that you will receive this indulgently. No article of the materia medica has probably undergone so many and varied experiments for this purpose, as well as for ascertaining its virtues, as the bark.

In the early periods of its use, the ill consequences that followed its misapplication, or indiscriminate use in every malady that bore the slightest analogy to those which had yielded to its influence, procured it many enemies; while latterly, the inferior quality or condition of the article itself, has had no less weight in producing a very considerable luke-warmness in its character.

The prejudices arising from the first cause, were dissipated in proportion as the improvements in pathology advanced. Since many diseases, exhibiting apparently the same weakness and general phenomena, are now well known to proceed from two opposite and contrary conditions of the system, the one requiring increased, the other diminished tone and action; hence directing the physician's attention to the existing error instead of the name of the disease; it seems probable that we have arrived nearer to certainty in the application of this, as well as we have done in the use of the lancet, and many other material remedies. But the indifference depending on the second cause has still much reasonable foundation. As the inferior quality and bad condition of this article are predominating with increasing effect, and seem likely to diminish, if not destroy its importance to the healing art, I think it cannot be deemed useless to investigate the causes of this depreciation.

Before the immense importations of inferior and spurious kinds of bark with which our markets abound(and which the relaxed state of Spanish authorities in South-America have favoured) we had but little cause to complain of their quality; but in the present state of this article, it is no less necessary to discriminate in the choice than it is in its administration.

Until about the year 1790, there were only two species of bark in use, to wit, the pale and red. Soon after that period, a third species, the yellow, was introduced; and at this time there are nearly a dozen different varieties, principally of the yellow kind, selling under the boasted title of Cinchona. About the year 1790, the exorbitant price of red bark encouraged a spirit of adventure in search of that article, which brought into this country a superabundance, a considerable proportion of which, although agreeing in colour, with the genuine, was found to possess but little medical power. The same causes have more recently produced a similar result in procuring the introduction of many species and varieties differing greatly in colour and quality.

The great difference in the size and thickness of the species, in texture, strength and sensible qualities, as well as in colour, afford the most conclusive evidence, that many different trees, bearing a greater or less degree of affinity to each other, furnish the different barks of commerce at this day.

To point out the uncertainty of relying on the bark as it is now generally met with ; to state some experiments made on different kinds, and preparations deemed of most practical use, without detailing those minuter ones which have been so often repeated by abler epxerimenters, and with such different results ; to point out rules for selecting ; the best mode of pulverizing and otherwise preparing the bark for exhibition ; and to invite your attention to some preparations of my own, is the object of the present address.

It is important in selecting bark to know, that its qualily depends more on its texture than on its colour.

Every piece of Peruvian bark, of whatever colour, texture, or quality, exhibits three laminae, viz. the exterior or epidermis, the central or parenchyma, and the interior or semiligneous. These appearances are more distinctly observable in the yellow than in the other species of bark, particularly in the inferior varieties, in which one half or more of the inward surface appears to be composed of strong parallel fibres, of a woody nature, difficultly reduced to a fine powder, and possessing but little sensible properties. The great or small proportion of the parenchyma, if sound, compared with the woody portion, serves as the best criterion for choosing bark in the crude state. In the best parcels, the parenchyma prevails nearly throughout the whole thickness of the pieces ; this breaks short and brittle, and appears dense. These appearances generally attend both the pale and red, but with some exceptions.

The epidermis of the pale and red is so thin and so interwoven with their corrugated surface, that no attempts have been made to separate it, the whole mass being ground promiscuously together. But the epidermis (which is an inert and useless substance) being thicker on the yellow, and adhering but loosely to its smoother surface, is principally separated from it before it is exported from South-America, leaving the parenchyma encumbered only with the woody portion.

A particular examination of the following species and varieties which constitute nearly all the different kinds now employed under the name of Peruvian bark, will serve to throw some light on the general nature and comparative value of each. For the sake of reference merely, I have thrown them into numerical order.

N. 1. Superior red. This comes in large thick pieces, has a thin epidermis, which adheres firmly to its rugged surface; is heavy, hard, and brittle, and breaks short and shining. The parenchyma is thick, compact, and plainly distinguishable from the inner semiligneous portion; taste strongly bitter and astringent; smell somewhat peculiar although common to bark; yields under a particular mode and given degree of grinding, a large proportion of impalpable powder, though less than the pale.

N. 2. Inferior red. Light and spongy; comes with little or no epidermis; parenchyma and semiligneous portion not easily dstinguished. This, from the deficiency of its sensible properties and its exceedingly tender structure, appears to be a decayed bark. The whole substance is reduced more easily than any other to a fine powder. This and the preceding, owing to their scarcity and the high price they both have born, are now rarely met with in their genuine state.

No. 3. A firm bark; heavier than any of the others; general thickness from one to three lines; external surface hard, smooth, and principally without epidermis; parenchyma dense and brittle, and constituting nearly the whole substance; semiligneous portion scarcely visible, and breaks with little or no shivery appearance; taste intensely bitter, and considerably astringent; colour in powder a yellowish red, approaching more nearly than any other to the true red bark; smell brisk and common to bark; yields more impalpable powder than any other, except the decayed red last mentioned. This appears to be a strong and valuable bark, lately introduced, but owing to its high price and better demand in Europe, but little has been retained here, and is probably not to be found pulverized in the shops of this country.

No. 4. Common pale bark; has a thin grey coloured epidermis, which is interwoven with its uneven surface; is mostly rolled up in short thin pieces. In texture it nearly resembles the superior red. The fracture is short, smooth, and shining; parenchyma thick, and scarcely distinguishable from the semiligneous portion; taste bitter and astringent; smell common to bark, but stronger than in the others. Although a hard bark, it affords a large proportion of impalpable powder.

Another kind of the same colour has been noticed, differing but little from the other, except in the size of the pieces, which are larger, thicker and wider.

No. 5. First quality of yellow; in large and small flat pieces, from one half to two lines in thickness; surface slightly corrugated: epidermis very thin and easily separated; parenchyma extending nearly through the whole substance; breaks short and brittle, but not shining, with but little fibrous appearance on its inner surface; colour a rich lively yellow; taste strongly bitter, with slight astringency; smell common to bark in general, with the addition of a slight cinnamon flavour, especially when heated; is capable of yielding a large proportion of impalpable powder.

No. 6. Second quality of yellow. The pieces are thicker and longer than the last mentioned, but not so broad, except a few pieces which appear to come from the tree near its root; thickness from one to three lines; surface shriveled; epidermis thin and adhering closely; parenchyma thick and scarely distinguishable from the more ligneous inner surface; breaks short, but not so bitter and shining as the pale; yields like the former, a large proportion of impalpable powder; taste biter with slight astringency: smell brisk and common to bark, without aroma; colour a pale yellow, approaching to the colour of the pale bark.

No. 7. Third quality of yellow; generally rolled up in the quilled form in larger and longer pieces than the pale;

surface somewhatsmooth ; epidermis very thin, light coloured, and adhering firmly ; parenchyma thin, hard and brittle, and easily distinguished from the semiligneous portion, which is thick, tough and shivery ; thickness from one to two lines ; taste pure, but not strong either in bitterness or astringency ; colour a yellowish ash ; smell common to bark ; yields but a small portion of impalpable powder, the remainder being very fibrous.

No. 8. Fourth quality of yellow. Of this general description, several varieties have been examined, all of which agree so nearly in texture, in the proportion of their component parts and in their sensible properties, as not to require a separate description. General thickness from two to ten lines ; epidermis thick, soft and adhering loosely, with signs of decay; parenchyma about one third of the whole substance, tender and spongy, and readily distinguished from the semiligneous portion, which is light, very fibrous and shivery ; colour in powder a reddish yellow ; taste feebly bitter, and very slightly astringent, and approaching to the taste of rotten wood ; smell weak, with a mixture of flavour common to bark and the faint smell of rotten wood ; yields very readily a small proportion of impalpable powder, while the rest remains shivery, lighter coloured, woody and fibrous.

Another kind of bark deserves to be noticed, more on account of its similarity in appearance when powdered, to some of the others, than on account of any real resemblance in medical virtues, and because large quantities have been noticed in market. It comes in pieces of very uniform length and breadth, which appear to have been sliced from the tree while in sap ; thickness in general from one to three lines, but in some pieces as much as six. In general appearance while in quill, it has a nearer resemblance to the bark of the birch tree than to any other ; colour in powder a reddish ash, inclined to yellow ; external surface hard, and in the larger pieces full of transverse fissures ; smell not common to bark ; taste scarcely bittter, but powerfully astringent. Different from any kind of Peruvian bark, it gives out to cold water a brownish red colour. It will not cure an intermittent. A cold infusion of it differs from a similar infusion of real Peruvian bark, in remaining like oak bark much longer, free from putrefaction, in producing a ready and copious precipitate with a solution of gelatin, in forming no precipitate whatever with infusion of galls, as the latter does, and in striking with chalybeates a much darker colour. When treated with water and alcohol, it affords nearly twice as much extract as Peruvian bark; which, however, is by no means to be considered as an evidence of superior strength as a medicine, since oak bark, galls, cinnamon, bark of guaiacum, sassafras, and many others, afford a larger extract than Peruvian bark, yet no one ascribes to any of them a tonic power equal to the bark. It is only when the constituent principles of several barks are the same, that the quantity of matter extracted from each by any given menstrua, will serve as a criterion of their relative strength.

The powdered state of all the barks being more favourable than the crude or unground, for judging them by their taste, smell or colour, they are to be understood, when their sensible qualities are here spoken of, as being examined in that state: and the proportion of impalpable powder which is here stated to be the product of particular described barks, is to be understood as meaning the proportion to be obtained under a given degree, and according to a particular mode of grinding and sifting, as is hereafter pointed out for obtaining, in a degree, the resinous or parenchymatous separate from the shivery or semiligneous portion.

To obtain concentrated preparations of the bark has long been considered a desideratum in pharmacy of the first magnitude; but none of its preparations hitherto made, have been found equal in medicinal power to the powder, and no solvent so capable of acting on all its component principles, as the juices of the primæ viæ.

The middle layer or the parenchymatous portion of the bark is that part in which the active principles chiefly abound. To separate this from the more bulky woody matter with which it is blended, has incited the ingenuity of many skilful experimenters ; but owing to the wrong application of pulverizing machines, little progress has hitherto been made towards the completion of this object.

The component parts of many bodies, though intimately blended when endowed with different degrees of friability, are capable of being separated from each other by mechanical means.

Different opinions have been held as to that portion of bark which soonest yields in pulverizing. While some contend that the resin or parenchyma is soonest reduced, others are of opinion, that on account of its apparent hardness, it is the last which becomes a powder. It is difficult to reconcile opinions so contrary to each other, when they are sanctioned by such names as Lewis, Duncan, Coxe, Woodville and others of great authority, without supposing that either very different modes of pulverizing were adopted, or that barks differing greatly in texture, were employed by each.

Resinous hard and solid bark, when triturated with the pestle, adheres more or less to it and the mortar in which it is ground, and the longer the friction is continued in this way, and the smaller the quantity employed, the greater will be the agglutination of the best part of the substance. In this case, a certain portion of the resin gets into hard and solid masses, while the ligneous portion seems soonest to be in the powdered state. But if bark of the same description be placed beneath the operation of two vertical stones of modeate weight, turning within a small circle, and in sufficient quantity to preserve a separation of the rolling stones from the plane beneath, of one inch or more, until the operation is completed, a contrary result will follow. In this case the more resinous portion does not agglutinate by coming into close contact with the levigating bodies, and in consequence of its greater degree of friability, is sooner than the other reduced to a subtil powder, while the remainder, still preserving the fibrous state, is more slowly reduced. To favour the separation of the fine, more spherical figured particles which are the product of the pulverized parenchyma, it greatly promotes the design to have the sieve preserved in a horizontal position, and to have the necessary agitation performed by motions in that direction. By these means most of the oblong fibrous particles, however minutely divided, by lying across the fine meshes of the sieve, are prevented from passing. When, therefore, the object is to obtain from the general mass of bark employed, a certain portion of its substance separate from the other, in a stronger and finer state than the remainder, the mode of both grinding and sifting last described is best adapted to promote the design. The superfine powder thus separated is observed to be of a much higher colour than the residue. This by further grinding may be reduced to an inferior powder, or may be advantageously used for tinctures, infusions, or for the preparation of the essential salt hereafter mentioned.

On the contrary, when the object is to obtain an uniform powder of the bark, consisting of all its component parts, the heaviest weights are to be employed, and to accelerate the grinding, are made to operate on small quantities at once, until it is uniformly crushed and sufficiently broken to pass chiefly through the bolt. The bolt, for better dispatch, is to be constructed and acted on in the same manner with those employed in factories of grain.

Thus, the different modes of obtaining different qualities of powder from bark of a given kind, are made to consist in the different degrees of weight and force employed, and in the manner of their application, whether on large or small quantities at once, whether by simple trituration, or by the combined action of pressure and trituration; while the different mode of separation are made to consist, not only in the fineness of the sieves, but in the manner of applying the pulverized mass to the sifting machine, whether this be done on an horizontal plane, with agitation in that direction, or whether it be done by means of the customary bolting cloth, with circular motions and perpendicular agitations.

The term resinous portion is here used in the broad sense in which it has been used by most writers, to mean that middle, brittle, and parenchymatous portion, which contains the largest proportion of its active parts.

All the descriptions of true bark herein noticed, have been repeatedly administered in the course of many years practice, with a particular reference to the comparative strength of each, as well as to the comparative value of the powder obtained in different ways ; from which I am enabled to say (as far as the varying circumstances of the different cases in which they were used would allow) that the efficacy of those which have been analyzed, corresponds with the quantity of concrete obtained from each, as exhibited in the following table.

Of No. 1. 2 and 7, it is regretted that the samples on hand at the time the others were submitted to experiment, were not sufficiently large to test them likewise by means of the concrete they might afford ; but their comparative value has hitherto been determined by their efficacy in actual practice, and by their sensible properties.

No. 1 is high in its sensible qualities and powerful in its operation.

No. 2 is inferior in sensible qualities and feeble in medical virtues.

No. 7 is not high in sensible qualities, and on account of its tough and fibrous texture is rarely made fine enough to be valuable in powder.

In each of the following experiments one ounce avoirdupois of bark was employed. The quantity of water poured on in each infusion was one pint. The mixture was stirred occasionally for three hours, and then allowed to settle twenty-one more. The decanted liquor was passed through filtering paper, and the evaporation immediately commenced in shallow earthen plates set either in the sun or in an open oven heated to a temperature below 150° of Fahrenheit, and gradually reduced towards the close of the evaporation, until the product assumed a shining vitreous appearance, somewhat similar to dark coloured gumarabic. In the experiments with proof spirit, half a pint was poured on the residuum left after three watery infusions; the mixture was agitated daily for eight days and decanted on the tenth. The menstruums employed were of the temperature of the atmosphere. Many trials were made with tepid and hot infusions, but with little increase in the quantity of the product. Decoctions increased the quantity but injured the quality. The addition of vegetable alkali, magnesia, lime and ammonia to water in different proportions did not appear to increase its solvent power, but changed the taste of the product to a nauseous bitter.

	Quantity of concrete extracted by water in grain.				Concrete extracted by proof spirit after water in grains.
	1st inf.	2d inf.	3d inf.	Total.	
Superfine powder obtained from No. 3,	45	18	8	71	
Proof spirit on the residuum after the three watery infusions,					25
Remainder after the above superfine had been sifted out,	38	15	3	56	
Proof spirit on the residuum after three watery infusions,					20
Superfine powder obtained from No. 4,	35	15	1	51	
Proof spirit on the residuum,					21
Remainder after the above superfine had been sifted out,	30	12	2	44	
Proof spirit on the residuum,					17
Superfine powder obtained from No. 5,	43	18	7	68	
Proof spirit on the residuum,					20
Remainder after the above superfine had been sifted out,	33	14	2	49	
Proof spirit on the residuum,					13
Superfine obtained from No. 6,	40	13	2	55	
Proof spirit on the residuum,					17
Remainder after the above superfine had been sifted out,	28	9	3	40	
Proof spirit on the residuum,					13
Superfine obtained from No. 8,	14	6	1	21	
Proof spirit on the residuum,					14
Remainder after the above superfine was sifted out,	9	3	2	14	
Proof spirit on the residuum,					9

The substance obtained from bark, when treated with water as above, has acquired in Europe the name of Essential Salt. How far the addition of the spirituous concrete obtained after water, may add to the value of the salt, and whether its value will compensate for the expense of preparing it, remains to be determined by further experiments.

This preparation I believe has not as yet entered much into the prescription of the physicians of this country, or been the subject of as much attention as it seems to merit.

Dr. Reece, in the 4th London edition of his work entitled " Medical Guide," has noticed the salt in terms of high commendation. He says it has been extensively employed by himself and many of the principal physicians in Great Britain; and in cases where the powder cannot be used, it is considered an article of inestimable value. Ten grains are stated by him to be equal to one drachm of the best powder.

" The tincture of the bark, on account of the quantity of spirits necessarily employed, is in many cases inadmissible; while a dose of the infusion, adequate to ten grains of the essential salt, would be more than the stomach would retain, or the patient be persuaded to swallow. From these great objections this salt is exempt. Its being soluble in small quantities of either vinous or aqueous menstruums, and its convenient form for being used in pills, renders it extensively applicable in cases where the powder cannot be duly administered.

This substance, like some of the deliquescent salts, absorbs moisture greedily, and after exposure to a moist atmosphere, it parts with its yellowish colour and vitreous appearance, and assumes, first, a gum-like; then a syrup-like consistence of a dark brown colour. The change thus wrought on this article by the admission of moisture, while it does not materially injure it as a medicine, destroys in a measure that characteristic appearance, by which its genuineness is to be judged of.

I find that a solution of the above concrete or salt, in the same quantity of water which originally held it in solution produces, on the addition of red sulphate of iron, the same succession of dark and green colours that were observable in the infusion before evaporation.

An infusion of galls, added to a fresh cold infusion of yellow bark, produced a copious white precipitate. A solution of the salt of equal strength with the infusion, on the addition of galls, let fall a precipitate precisely similar in colour and quantity.

On the addition of a solution of gelatin to a fresh cold infusion, little or no precipitate could be distinguished. The same result followed the addition of gelatin to a solution of the salt.

A solution of red sulphate of iron (fifteen grains in one ounce of water or spirit) when added to eight times its quantity of the tincture obtained from the residuum of bark after maceration in water, and which afforded the concrete in the last column, gives it a dark green colour and turbid appearance. When the concrete is re-dissolved in the original quantity of spirit, the solution undergoes the same change on the addition of the chalybeate.

The above tincture affords, on the addition of either infusion or ticture of galls, a very small grey coloured precipitate. A similar precipitate is deposited by a solution of the concrete on the addition of galls.

When water in a large proportion is added to the tincture above mentioned, a floculent precipitate is formed. When the concrete is redissolved in proof spirit, and water is added, a precipitate is likewise formed, similar to the other in appearance and quality.

The experiments made on these concretes, being calculated to ascertain the existence of several of those proximate principles on which the virtues of the bark are supposed to depend, prove, that they contain those principles unchanged. As to sensible properties, they retain much of the taste peculiar to Peruvian bark. It will be perceived that the principal difference between the mode of preparing this, and the common extract of the bark, depends on the application of moderate heat in preparing the one, and the higher degrees in the other. It is now generally admitted, that the great heat which is usually employed in making the extract by long boiling, destroys that just combination of its constituent principles on which its medicinal power depends. Does the great heat employed, the agitated state of the boiling liquid, or both united, by favouring the absorption of oxygen, produce this effect ?

Among the causes which have lessened the usefulness of the bark, its misapplication and depreciated quality have been noticed. Another cause of miscarriage has been owing to the small quantity administered. It is with this, as it is in the use of many other permanent stimulants (mercury, for instance) that no beneficial effect is experienced until the full quantity is given which corresponds with the lost excitement. If, instead of giving infusions and decoctions in the frivolous quantity of two or three spoonfuls, or the tincture by tea-spoonfuls, as is too frequently the practice even in cases of urgency and danger, the powder were to be administered in the dose of a drachm or more every hour or two, or if it were injected into the lower bowels in still larger quantities, less frequent complaint would be made of its inefficacy. Not a few are the number of cases wherein the invigorating power of the bark is most imperiously demanded, where the requisite quantity of the powder cannot be retained on the stomach. In nearly every case thus circumstanced, the desired effect may be expected from liberal injections of this substance. I have witnessed more sudden and complete effect from bark exhibited in this way than from any other. (This may be owing to the large quantity used.) One ounce or more of fine powder, incorporated with twelve ounces of starch mucilage and fifty or sixty drops of liquid laudanum (where that is admissible) will generally be retained, especially if mechanical pressure to the rectum be made for the first half hour. This quantity of bark, with thirty or forty drops of laudanum may be safely repeated three or four times in every twenty-four hours.

Intermitting and remitting forms of fever generally affect aged and infirm people dangerously, and when they rage epidemically are often noticed to be malignant, and more or less mortal to persons of every age and description. In cases like these, and many others, where the stomach cannot receive the requisite quantity without the debilitating effect of vomiting, bark injections become indispensable to the safety of the patient. I have known very many persons, from these and other causes, affected with coma, vomiting, cold extremities, paralysis of the sphincter muscles, almost imperceptible pulse, &c. speedily restored by the liberal use of bark in this way. Repeated experience furnishes the most conclusive evidence to my mind, that the human system is not less speedily and effectually acted upon by bark, in the appropriate quantity, through the medium of the lower intestines, than it is by means of the stomach: in fact, whenever this organ has lost much of its excitability by the too liberal and long continued use of ardent spirits, opium, strong and high flavoured tea, and other high stimulants, it is manifestly the part less adapted than the lower bowels, to impart to the whole body the effect of this and some other medicines. This pleasant method of administering medicine adapted to the excitability of the part, has, it is true, been noticed by many writers on medicine, but in so incidental a manner has it been done, as to render the hint almost nugatory. Thousands have doubtless been abandoned to their fate, because the bark could not be retained on the stomach; many of whom might, by this practice, have been saved to bless the great Author of all good for giving to mankind this divine remedy.

However extensively this invaluable drug may apply to the catalogue of human infirmities it approximates no neare to an universal remedy than the lancet does. Like that, its power of controlling a certain description of morbid actions renders it equally mischievous in those of a contrary state. Although extensively useful in intermitting forms of fever, it cannot be universally so. The intermitting and remitting forms of fever, as well as others, are now known to assume, in different places and seasons, and with different persons, very various and contrary characters. Whenever fevers of this description are characterized by a hard pulse, inflamed eyes, great pain in the head, stomach or intestines, with oppression of the breast, &c. no practitioner would think of curing them without having recourse to the lancet, and other depleting remedies. When symptoms of a contrary nature supervene, the lancet would be as prejudicial as the bark beneficial.

It is worthy of particular remark, that intermittent fevers rarely, if ever, assume at their commencemen the tonic or atonic character on which our indications of cure are subsequently formed. Those which might, by a repetition of paroxysms, assume in their further progress the tonic or inflammatory diathesis, are as certainly cured by the early use of the bark, as if no such predisposition to phlegmasia had existed. It is in this early stage of the disease, before the morbid actions of the system are habitually confirmed, or the glandular secretions are materially vitiated, that it is best subdued by the bark. The evacuation of the primæ viæ so generally, premised to the use of the bark, is here to be dispensed with, on account of the delay which it occasions, and because the state of the secretions does not require it. In that intermediate and doubtful state of the excitement which attends not only these, but some other periodical diseases, we may venture, with greater confidence, on the borders of the inflammatory state, with this remedy in our hands, than with any other tonic of the materia medica.

The yellow is that species of bark which is now most generally preferred in the United States. It is on this account, and because the best descriptions of this kind are thought to be equal, if not superior to those of other colours, that I have latterly had this kind powdered, and put into market. The superfine powder is obtained from that variety which we have denominated first quality of yellow (No. 5.) in the proportion of about two parts of the superfine to one of the remainder. The preparation has been more successfully employed (given in two-thirds the usual quantity) during several years of my practice, than any other heretofore made use of, and the testimonials of many other practitioners to whose judgment it has been confided, being equally in its favour, it is not on slight grounds that it is recommended to your notice.

The first account of the preparation and uses of the concrete or essential salt of the bark, was noticed in the London Medical and Physical Journal, vol. vii. The more particular remarks of Dr. Reece, and other physicians with whom he has corresponded, on the merits of this article, go far to encourage its use. I have given it a few trials in cases of distinctly marked intermittents, in the dose of ten grains to adults, in the form of pills, every two hours, during the intermission, with uniform success; and in several cases of chronic debility which have recently been under my care, it has had the happiest effect, when given by itself, or in combination with chalybeates. In consequence of the evidence thus furnished in favour of this preparation, I have been induced to construct an evaporating furnace for the purpose of preparing it in the large way; and should the result of the further experiments which are now in progress, not tend to invalidate the opinion already formed of it, I propose to supply those agents who may have charge of the superfine powder, with this article likewise.

After more than twenty-five years observation on the effect of bark, as a medicine, in different situations best calculated to give it an extensive trial, as well as ten years attention to the different ways of pulverizing and otherwise preparing this substance for use, it may not be thought arrogant to lay claim to confidence in regard to the facts and principles herein contended for. A few experiments, if made with attention to accuracy, as is here pointed out, and (what is a surer criterion) a few trials made with these preparations in well adapted cases of disease, may determine them satisfactorily.

Should the above remarks and observations, by exciting a spirit of further research on the subjects to which they relate, prove in that or any other way useful, it will answer the wish of

Your obedient servant,
CLARK SANFORD,

Greenwich, *Connecticut*, *June*, 1811.

Nathaniel Potter, M. D.

…………

To the Editor of the Lycæum.

SIR,

AFTER reading Dr. Dallam's paper in the first number of the Lycæum, I resolved to try the effects of the Cotton in burns, in the first case that occurred in my family; and it soon happened that a case presented itself, for one of my children was scalded with boiling water. The Cotton was immediately applied, and a cure was effected by it alone, without an ulcer or even a blister. There was nothing extraordinary in this case, except the shortness of the time in which the cure was performed. Several other cases have fallen under my observation, and in every instance with the same happy result, none of which it will be necessary for me to state except my own, which was far more alarming, because the accident was occasioned by a different agent in combination with the matter of heat possessing a greater capacity for heat, and of a more adhersive quality than common water.

On the twenty-fourth of June, as I was preparing a plaster for a horse's back, I had the misfortune to spill *boiling rosin* (one of the ingredients) over the back of my hand, from the wrist down to the ends of my fingers. Such was the excruciating agony of the pain, that I threw myself on the floor, in a state of distraction. I laid there about three minutes, when some grease was brought for the purpose of disengaging the rosin. By this application, aided by the heat of the fire, to which I held my hand, the rosin was got off in about fifteen minutes, during which time, I suffered extreme pain. The cotton was applied in about twenty minutes from the time I received the injury, and in half an hour after the application, I felt no pain except at the end of my little finger. There the pain still continued, except when I pressed the cotton close to the part, and then it immediately subsided. I could not give the same degree of pressure to my little finger, as to the other parts of my hand, unless when I occasionally used my other hand, and thus the cotton was not kept so uniformly in contact with the part affected. Twenty-fifth—Continued free from pain, except my little finger, which was blistered : this convinced me that the closer the cotton can be pressed to the parts the better. Twenty sixth —The cuticle of the little finger is broken ; but all the other parts are free from pain, and all appearance of disease. I this morning applied the cotton to the little finger only. Twenty-seventh and twenty-eighth—Free from pain. Twenty-ninth—Omitted the cotton altogether, and suffered much pain in my little finger. On the morning of the 30th, I perceived a yellow deep-seated speck, as if pus were beginning to form. I cut down to it and applied the cotton as before. July first, a very small discharge, but no pain. Second, omitted the cotton and continued well. It is to be remarked that the cuticle of the whole back part of the hand, fell off three weeks after the accident, although no vesication had taken place immediately subsequent to the burn.

Yours,

JOHN HASLAM.

Baltimore, September 24, 1811.

Hayden's Geological Sketch of Baltimore, &c.

Baltimore, July 12, 1811.

IN attempting a brief geological sketch of Baltimore and its vicinity, it seems unnecessary to wander far in search of proofs to convince the most superficial observer, that water has been the principal agent in forming almost all the inequalities of hill and dale, that present themselves to view either in an easterly, westerly, or southern direction.

The superincumbent soil in the neighbourhood of Baltimore, and even for many miles distant, (except in some interval) is a mixture of clay and sand, in which the former predominates; hence, it is not very productive in itself, but susceptible of the highest as well as most lasting state of improvement and cultivation.

After penetrating this stratum, which varies in thickness in different places, we next come upon a wave-like stratified bed, or vein of sand mixed with a brownish loam or ferruginous substance, which often presents to view in the side of a bank recently cut down, a very handsome appearance.

The declination from the horizon of this stratum, differs in its degree and direction, as to the points of the compass, but bears mostly the appearance of having been once washed by a current, the direction of which has been south or southwesterly. This stratum is occasionally intermixed with or interrupted by extensive beds of quartz and other pebbles of various colours and dimensions, which in some instances form the superincumbent surface; at others the second stratum, and in some the third; constituting with the following, four different strata. Immediately below the second stratum (or the third where there is a bed of quartz pebbles) we come upon an immense bed of very fine white silicious sand, presenting undulating shades, and occasionally exhibiting nodular masses, and sometimes considerable beds of very white argillaceous grit. This bed of sand is not only useful for a variety of domestic purposes, but is of importance to plasterers and bricklayers. It commences at from ten to fifteen feet below the surface, and extends to a great depth; perhaps at present unknown; yet there are circumstances that lead to a conclusion, that it is limited in its depth, at or a little below tide water. When the water company were digging the canal for the purpose of conveying the surplus water from the hydrant and new mill (belonging to the water company) into the present course of Jones's Falls, they were obliged to cut across the ancient channel of the above stream; which course it had left many years since, and to a depth, greater perhaps than has been opened in any part of the city. The bottom of this canal, which was from eight to ten feet below the surface, and five feet below tide water, and nearly one hundred below that on which the Cathedral is building, presented a bed of pale blue argillaceous grit, occasionally intermixed with veins of chloritic earth, or chlorite in a pulverulent state, forming a stratum extremely tenaceous and hard.

From this, and other indications, there is much reason to conclude that there are argillaceous strata, or beds of considerable extent, not only in the neighbourhood of Baltimore, but in various directions about it. Between the city and Fort M'Henry they are presented to view in different places, at tide water, of various qualities and colours; such as redish brown, dark brown, pale blue or slate, white, &c. some of which are of a very fine quality.

On the opposite shore at Moales' Point, and near Messrs. Dorseys iron ore tract (which is of the argillaceous kind) there are extensive beds exposed to view at tide water, some of which are excellent, and might be turned to considerable advantage to the manufacturer.

In numerous instances the mind is often very much assisted in forming an opinion of the geology of a district, by the various substances, which at different times, are raised from a very considerable depth below the surface. In this respect, however, very few opportunities have been presented, which are calculated to throw much light on the subject under consideration; one however, it may not be amiss to notice. Mr. ——— in digging a well in that part of the city east of Jones's Falls commonly called Old Town, came upon a mass of wood. evidently black walnut (juglans nigra) with a part of a nut of the same species, at the depth of twenty-one feet from the surface; both of which are deposited in the Museum in a good state of preservation. These, in all probability must have remained for ages at that depth below the surface, since there is no possible evidence of the current of Jones's Falls ever having overflowed that part of the city in any direction, it being from eighteen to twenty feet above the present surface of the stream.

The most conclusive evidence however, in favour of the agency of water in producing the present appearances (at and below the surface as far as we are acquainted) is a stratum of iron stone, which is considered, I believe at all times and in all places as an unequivical proof of secondary formation. In no instance, perhaps, can it be more strikingly so than in the present one. At from eight to ten feet below the surface of the eminence on which the Cathedral is at present building, and in some instances at a less depth, this stratum presents itself, and is in general from six to twelve inches in thickness, preserving in its extent, almost an uniform level, conforming however, to the inequalities of surface on which it was primarily deposited and formed. It is composed mostly of masses of from one to fifty pounds; presenting a great variety of forms; some thick, others thin, wave like, round, hollow, &c. I have seen a piece of it in the form of a complete hollow cylinder open at both ends. It is composed mostly of the argillaceous and silicious iron stones, with some indications of what I consider the carbonaceous kind: but in no instance that I am acquainted with, has the calcareous, bituminous, or granulated iron stone appeared among it. It preserves its level, or nearly so, until by the descent of the hill, it runs out, and is not seen again except on nearly the same level in a high neighbouring hill, as in several places in a westerly direction, and beyond St. Mary's College on the Reister's town road, near a mile distant from the first mentioned place. Detached masses of this substance are frequently seen in lower situations, and sometimes in considerable quantities, but I consider this as having, in the course of time, been torn from its natural bed by some physical cause, perhaps by torrents of water cutting through the hill and stratum, leaving the pieces to fall with the bank which in time is washed into the low grounds.

We next see its appearance about half a mile distant on Hampstead Hill, which lays due east from Baltimore, and extending with the hill in a south easterly direction and preserving the same level, until, from the declination of the eminence, it runs out near a mile below, or to the southward, in the vicinity of Harris's Creek. Its next appearance is in a direction due south from the city, in Federal Hill on which the observatory stands, and at nearly the same depth below the surface or summit of the hill, and preserving nearly an uniform level quite through it.

The next highest ground in the vicinity of Federal Hill is on the Patapsco, at Moale's Point, as before mentioned, about one mile distant, in a south westerly direction. Here it appears at a less depth below the surface than at either of the aforementioned places, the land being probably somewhat less elevated.

The above mentioned places comprise in the whole nearly eight miles in circumference; throughout which, as well as at many places where it is discoverable between Baltimore and Annapolis, which is thirty miles distant to the southward and about the same height; this stratum of iron stone preserves nearly the same uniform height above the level of Chesapeak Bay; a circumstance which, while it seems too obvious to have escaped the notice of the most superficial observer, amounts to the strongest proof that the neighbouring country to a much greater extent than I have noticed, is almost entirely of a secondary formation.

As it was not my intention in the first instance, to take a very extensive view of the surrounding country, I shall only observe, that to such as are fond of contemplating subjects of this nature, few opportunities will afford a more interesting field for speculations of this kind, than the one under consideration. If the observer will cast his eye over the extensive scene, and suffer his mind to dwell, for a time, upon the low and far stretched shores of the Chesapeak, fading in distance, if he takes into view the different rivers, their courses, the force and operation of their currents, at their several points of influx, together with the projecting capes, head-lands and sinuosities of the eastern and western shores, with their correspondencies, it would seem impossible for him to resist the impulse of astonishment and surprize that fills the mind, while contemplating the amazing results of operations begun and performed during the period of elapsed ages.

Having briefly noticed the appearances within, and near the city of Baltimore, I shall next offer some remarks on a portion of the same district, which has perhaps, a much higher claim to antiquity than the one above described.

It is a part of the granitic ridge, which lies near the city, and on the foot of which, it may be said, Baltimore stands. But before I proceed to describe the appearances of a part of what is called the Granitic Ridge, it may not be improper to offer some general remarks in relation to its whole extent.

The course of this immense ridge, is mostly in a north-easterly and south-westerly direction; varying however, to almost a due east, and west course, in others, almost north and south. Its southern extremity is at or below the state of Georgia, and extending in the above direction, north-easterly through a great portion of the United States. The Roanoak crosses it at Halifax; the Appamatox at Petersburg; the James River, at Richmond; the Rappahannock, at Fridericksburgh; the Potomac, at Georgetown (Col.) the Patuxent, at or near Snowden's, on the post road to and from Washington; the Patapsco, between ten and fifteen miles from its influx into the bay; throughout a great part of the above distance, it is obscured by the incumbent soil. It appears again nearly in its whole breadth at Gwinn's Falls, five miles from its junction with the Patapsco. From thence it mostly disappears under the soil untill it is intersected by Jones's Falls, where it appears in most of its breadth. From thence its course is continued through Baltimore and Harford counties, and appears on both sides of the Susquehannah at a great height, and more rugged form than any part of its range with which I am acquainted. From the Susquehannah, its usual course is continued with occasional interruptions, as at Schuylkill, to Trenton. From thence it is continued across New-Jersey to the Hudson River, where, I believe, its highest point appears at Hoboken. Thence it crosses York Island, and appears again, at above and below Hurl Gate, on the East River, (a part of Long Island sound) on the western shore of which, it forms some of the most beautiful situations, and affords, at the same time, some of the most interesting prospects of any that present themselves on the Atlantic coast; Greenfield Hill, (Conn.) excepted.

From Hurl Gate it ranges along the north shore of Long Island, nearly or quite to its north-eastern extremitity, where we lose sight of it by the channel, or passage which runs between Long Island and Fisher's Island. After passing under this channel, it discovers itself again on the Western-Shore of Fishers Island, and at low ebb tide at some considerable distances from the shore; constituting, I believe, what is commonly called Fisher's Island reef, or Race Rock.* From thence it continues its course, rather more northerly across Fisher's Island, and passing under the Rhode Island passage through the sound, strikes into the Mainland, opposite the north or northeast end of Fisher's Island, a small distance below Stonington, (Conn.) at a place called Watch Hill, and there constitues doubtless, what is called Watch Hill reef; beyond which, I have neither a general or acquired knowledge of it.

---

*Having had frequent opportunities of observing this reef, and that of Watch Hill, in sailing round them, I have been led to form this conclusion, from the obvious correspondenee in their course and direction, and that of the ridge which ranges along the northern shore of Long Island. However, not having had an

This extensive Granitic Ridge, when viewed in its whole range, forms one of the most interesting geological tracts that presents itself to view in any part of the United States; particularly so, when that part which is comprised within the limits of from Hurl Gate, to the north-eastern extremity of Long Island, and through Fisher's Island to Watch Hill, at Stonington (Conn.) is duly considered.

When all the concurring circumstances are judiciously arranged and duely considered, it is difficult to resist the force of evidence which is offered in support of the opinion, that this part of the Granitic Ridge, once formed the southern boundaries of the states of Connecticut and New York; and that their present winding shores, on the north side of the sound were once extended, gently sloping, to fertalize its base on the northern side, while its base on the southern side, throughout nearly its whole extent, (which in some places is, at present, more than two hundred miles from the sea) were once subject to the ceaseless howling of the insulting billows of old ocean.

Having finished what you may consider an unreasonable digression, I shall resume again the subject of its appearance at or near Baltimore, as viewed in a transverse, or rather oblique direction across the ridge; not neglecting to notice at the same time, whatever may appear interesting in a mineralogical, as well as geological point of view.

opportunity of examining particularly the formation and texture of the masses which compose those two reefs, (except that of being driven in a ship upon that of Watch Hill, which afforded but a poor opportunity for making geological observations) I may, possibly, have been led into an error; for which, I hope this will be a sufficient apology.

The foot of the above ridge, on the south side, appears within the precincts of the city, immediately in front of the county goal, and there forms the bed of Jones's Falls; and probably extends southerly, constituting a complete substratum to the city of Baltimore.

The texture of the general mass is at this place, that of a fine, hard, and compact granite, mostly stratified, exhibiting layers of from two to twelve or eighteen inches in thickness, having a declination, from the horizon of from thirty to forty five degrees. Over this bed, the current has flowed perhaps for ages without producing any material change, except that of forming deep hollow cavities, occasioned by the constant attrition of quartz, or other pebbles. Immediately on the margin of this ridge of granite, there appears a secondary ridge laying in a corresponding direction, composed principally of chlorite* intermixed with occasional masses of the black oxide of maganese and a ferruginous matter.

As we ascend this ridge in the direction of the falls, this granite gradually changes its texture by almost imperceptible gradations, until at the distance of one mile from its southern border, it runs into a coarse kind, where the fel'dspar predominates, so as to render it unfit for the purposes of building, or other branches of masonry.

Immediately above this, it assumes nearly the texture of the first mentioned, being fine, hard and compact, and gradually passing through the above transitions, until, at one mile and a half from Baltimore, it acquires a texture, such as to render it highly valuable and useful in various branches of masonry, and as such, is here quarried on both sides of Jones's Falls to considerable advantage to the proprietors.*

---

* This ridge of chlorite lies but a few rods above the canal before mentioned, where the veins of chlorite appeared among the argillacious grit.

† Independent of the great advantages this ridge affords, in different places to the various purposes in building, &c. it has been, and ever will be of no small importance to the several road turnpike companies, and would be of still greater utility were they

At this place its highest point is about sixty feet above the surface of the water in the creek, and is decidedly stratified throughout in layers of from two to thirty six inches in thickness, presenting various degrees of declination.

Here, are presented a variety of other substances that mostly accompany granite. Such as veins of pure quartz, feld-spar and quartz, the former of various colours; feld-spar quartz and granits, adularia, or moon stone, beryll, &c.

to confine themselves more to the use of such materials as it affords, and less to the indiscriminate use of such as present themselves in different parts of the country through which the roads pass, of which there are at present, no less than four, runing from Baltimore in different directions, and crossing this Ridge.

It is not my business in the present instance, to point out, if I were capable of doing it, the variety of substances that ought only to be used for the purpose of making roads; or those on the contrary, that ought not to be used.

Suffice it to say, that there are a great variety of substances that are of themselves, when exposed to the air, &c. decomposable, or easily ground into sand or dust; so that when such materials are used in turnpiking of roads, they soon experience one of the above changes; and the consequence is, that of necessary repairs. This being done, probably, with the same kind of materials, being nigh at hand, or in the neighbourhood, soon renders the second repair necessary; consequently, repeated and heavy draughts on the company's treasurer, until the receipts are nearly exhausted, and there remains little or nothing for the stockholders. Whereas on the contrary, if due attention was paid to the selection of such materials for the purpose of turnpiking, as the country abounds with, and which are indistructible, the roads, instead of wanting the usual amount of repairs, would be every year growing better, and no injury to be apprehended from any other source, than that of irrisistable torrents of water.

This want of a little attention sir, is visible in a greater or less degree on most of the turnpike roads, and even in paving the streets of our cities, which often helps to increase the burden of expenditures I have repeatedly seen the article of maltha, (a bituminous substance generally brought with ballast,) made use of in paving in this city; which you are sensible, is but little better than a loaf of bread for that purpose, for as soon as a cart or waggon has passed over it several times, it is ground to powder or dust; the adjoining stones losing their support, give way, and soon renders repairs indispensable, and additional expences unavoidable.

of which, handsome specimens may be obtained. Also, veins or beds of schistose mica; among which is intermixed considerable quantities of martial pirytes handsomely variegated, presenting almost every variety of colour. Besides which, there is a vein of the shorlaceous actynolite (of Kirwan) both in a crystalized and amorphous state.

This substance shews itself at four different places beside, within the distance of nine miles on this ridge and in a stratum not much thicker than the hand. At Gwinn's falls, it is accompanied by the lamellar actynolite—the fibrous zeolite, the viridescent, plumbaginous iron ore in green martial earth, &c. &c.

Ascending the ridge still farther, the mica predominates in the mass of granite, to such a degree as to render it unfit for any useful purposes. In this part of the ridge particularly, and in its neighourhood may be found handsome specimens of black tourmalin, crystallized in regular sided prisms.

Above the last mentioned part of this ridge, the transition is gradual, and continues with occasional difference of substances to its termination, or rather, where it disappears again under the soil, three miles from its southern border or margin. Here, as well as for some distance below, the mica and fels spar predominate in an eminent degree, particularly the latter, which is in considerable masses. This part of the ridge is not stratified, but lies rather in huge, detached masses without order or regularity.

Here it may not be amiss to notice a peculiarity which is observable in this part of the ridge; I have observed that its course was in some instances almost due east and west; at others almost north and south. An instance of this kind is visible at this place and at Gwinn's Falls. The stratified part of the ridge, or southern half has an inclination of nearly forty-five degrees west of south, while the unstratified or northern part runs nearly in a westerly direction, and seems to terminate abrubtly at Gwinn's Falls, at the distance of three miles, (I should judge) from and above the Frederick Town road. The above course of the stratified range is continued in its south westerly direction until near Gwinn's Falls, it seems to take a direction of about twenty degrees west of south, while the stream or current (the course of which above the ridge is north and south) runs over its bed nearly at a right angle across the ridge, or quite in an eastern direction; after which is assumes a course nearly south; while the ridge, in part is continued down on the west side of the stream and passes the Frederick Town road, a small distance from the bridge erected over the falls at Messrs. Ellicott's mills.

Besides this, in several other places, parts of this ridge appear running almost due south, while the strata appears to lay, or rather stand almost perpendicular to the horizon.

Immediately on the disappearance of the granitic ridge on the north side, and between Jones's Falls and Gwinn's Falls it is succeeded by extensive ridges, or veins of chlorite, intermixt with a variety of other substances, among which are found handsome specimens of the tremolite (or grammatite of Haüy*) and several other substances, interesting in mineralgy.

On the disappearance of the above ridge on the east side of Jones's Falls it is very soon succeeded by indications of serpentine occasionally intermixed with traces of the noble serpentine. This is seconded by an inconsiderable ridge of granite; a little to the northward of which we meet with stratites, in a range which yields the greatest abundance. This again is succeeded by what is called the Bare hills; and through which the Falls' Turnkike road has recently been cut.

The range of these hills is nearly in an easterly and westerly direction; not high nor much broken. The eastern part of the serpentine range terminates within a small distance of the creek which is called Jones's Falls. On the north side they are bounded, in part, by an auxiliary branch of the same creek. When ascending them on the south side, nothing strikingly interesting presents itself in the general view; but when approached from the east side, or viewed from their summits at different points, or from the road on the north side, the mind seems involuntarily to feel the impulse of melancholy. For the thinly scattered soil, which has been, perhaps for ages, slowly accumulating from a few decayed vegetables which some friendly breeze has borne upon its barren surface, is scarce sufficient to nourish a few scattered tufts of grass. A gloomy silence pervades around, while every rood on the serpentine range bears the most decided marks of sterility. The stones which cover its surface in many places, and of which the hills are mostly composed, bear the evident marks of a partial decomposition, which gives the whole a smoaky, brown appearance, as if it had been subjected to perpetual sulphurous exhalations, or steams of subteraneous fires. If a fratricidal Cain had here committed his Heaven insulting deed, we need not wonder; it bears so much the appearance of having experienced the unappeased wrath, the eternal curse of an offended Deity. Scarce a solitary lizard, or a creeping ant is here seen, seeking among the mouldering serpentine a peaceful borough in which to deposit its daily spoils, and screen its tender offspring from the chilling blasts of winters wind, which sweeps in triumph over this dreary waste. Besides the interest which it excites on account of its gloomy aspect, few districts present a more interesting field in a mineralogical point of view than the one under consideration. The principal eminence appears to be composed mostly of an inferior kind of serpentine, even that which is raised from the depth of forty feet below the surface, is of a loose, and somewhat friable texture; and at the same time intermixed with a variety of substances of the magnesian genus; such as the amianthus, asbestus, steatites, common and foliated; carbonate of magnesia, amorphous and crystalized; a species of lardite; noble serpentine, and several other interesting substances not sufficiently defined.

---

* The tremolite is also found at Beaver dam, thirteen miles from Baltimore on the York road, in a grangue of pure granulated carbonate of lime. This is so singular that it is particularly noticed by the Mr. Haüy as being found only at the lake of Baikal in Siberia, engaged in the gromulated carbonate of lime.

See Haüy's Traite de Mineralogie, Vol. 3, page 233.

But the most important substance that has yet, I believe, been found at this place, is the chromate of iron, of which there are immense quantities.

This interesting substance, of which much has been said in different parts of the world, appears at this place evidently under three different forms, or rather different states. At the place where it has been principally sought for and raised for the different purposes to which it has been converted, it appears in masses pretty compactly formed and of various sizes, as it may happen to be broken up, and among which is intermixed more or less of talc, steatites, &c. Its colour inclining to black, or dark brown. Its fracture, in some instances appears shattered, in others, conchoidal, and in others somewhat foliated, presenting smooth surfaces, having some what the resemblance of brown blend, for which this substance was mistaken in France, by citizen Pontier, who first discovered it; but which, by the analysis of citizen Tassaert, was proved to be the chromat of iron.

At the same place it is also found perfectly granulated, in a gangue apparently of indurated steatites, yet considerably harder. In this substance it appears in innumerable grains of an irregular form, separate from each other, and from the size of a grain of mustard to that of a kernel of pepper, though the latter are rare.

In the next place it is found crystallized in perfect and well defined crystals of an octoedrical form, and from the size of a grain of mustard to those whose facets are nearly one eighth of an inch in length.*

Those crystals have been observed, as yet, only on the eastern part of the hill which constitutes the serpentine range, about one quarter of a mile east from where the amorphous kind is raised or dug up. On this part of the hill are several considerable ravines or gullies, occasioned by water, which, in the course of time, has worn the serpentine, in some places to a considerable depth. In these gullies, the crystals are found washed down and mixed with sand, and a considerable quantity of the granulated chromat of iron.

---

* Those crystals have, for some time, been considered as octoedral iron ore; but they have recently been tested in this city, and proved decidedly to be the chromat of iron.

The exact locality of those crystals, and the granulated chromat of iron that accompanies them at this place, I have not as yet been able to determine; but, from some indications, I am strongly inclined to believe that they were once imbedded in the thick veins of green indurated talc, which, in many places in the sides of those gullies, runs through the serpentine.

Since crystallized chromat of iron is, as far as we have any knowledge, a novelty in the mineral kingdom, permit me to hazard a few remarks on its characters.

Its colour is mostly black, inclining in some degree to steel grey; when held in a bright light it often presents the colours of tempered steel. Surface of the facets of the crystal, untarnished, and smooth as if highly polished. Transparency 0, except in some instances when laid upon a sheet of white paper and in the light of the sun, a rich, deep blood red colour is perceivable. The irregular fracture of the crystals, conchoidal, and vitrious, treated with the blow pipe, infusible without addition; with borax, it gives a bead of a beautiful green. Rather brittle and easily broken, as must appear, for many of the crystals are mutilated, probably by the pebbles rolling over the rocks, and falling on them.

Magnetic in a considerable degree.

Pulverized, the powder or dust is of an ash grey.

The form of those crystals, although perfect octoedrals with regular pyramids in many instances, it does not follow that they are uniformly so in all. In some, the angles of the pyramids are considerably more sallient than in others. In some, the base, instead of being rectangular, is sometimes rhomboidal. In others, the bases, though rectangular, are oblong. In others, one of the pyramids is considerably depressed or flatttened; with others, both are flattened.

The primitive, integral form of those crystals, is tetraedral. This may be ascertained by any person who is desirous of trying the experiment; for the crystals are divisible, parallel only to the facets of the pyramids.

Immediately to the northward, as well as to the eastward of the bare hills the-lime stone commences. This, I believe is its first appearance in the vicinity of Baltimore, which is distant six miles. From this to the distance of twenty miles to the northward, and how much further I am unacquainted, the lime-stone tracts discover a variety of transitions.

In many places, it approaches so near to marble, as to render it not only useful, but highly valuable in almost every branch of civil architecture; and the prospect is favourable, to a supply of such as will answer every purpose of statuary and sculpture in all their variety. I have a piece that was raised at the distance of twenty miles from Baltimore, of exquisite whiteness, fineness and beauty; at which place the lime-stone runs side by side of a ridge of primitive granite, among which are found varied and beautiful specimens of cyanite (or sapparre of Kirwan) such as the blue, green, silvery-white, pale blue, &c. &c. also indurated iron ore in considerable masses.

In the distance above mentioned, there are a variety of other substances not enumerated; among which are the oxide of Titanium, also the Hemartites in beautifully varied forms.

After passing the granitic ridge to the westward, at the distance of five miles from Baltimore, on the Frederick Town road, we come upon a tract, interesting not only in its nature, but for the variety which it presents to view.

It consists of a ridge or vein of agate, varied however in its course, to all the different substances which enter into the composition of agate; such as clear pollucid quarts or crystal, calcedony, jasper, &c. &c. and of various colours.

What other appearanees this vein may discover in its whole extent, I am unable to say, but at the place above mentioned it presents handsome specimens of fine spotted agate, both disseminated above the surface, and in seemingly, stratified ridges or rocks,* appearing a little above the surface.

Its course or bearing is north, about twenty degrees east, and is, I believe, on an avarage, not more than from forty to fifty yards in breadth, in the distance of twelve miles, which is the extent of which I have any knowlege of it.

There is much reason to believe that this vein extends to a great distance to the southward, even to Georgia. For the emethyst is found in Virginia,† and the bur stone that has recently been discovered in Georgia (which is principally composed of marine shells) contains calcedony, and many of the cavities when broken open, presents a beautiful druzy incrustation of jasper and emethyst.

At about three miles in its direction northward of Frederick Town road, the agate is less prevalent, and the calecdony seems to predominate with occasional masses of handsome yellow jasper, susceptible of an exquisite polish.

The calcedony is in considerable masses, and on breaking open a piece, it presents numerous cavities of different dimensions, lined with crystals of quartz occasionally coated with a druzy like paste of a beautiful snow white, yellow, violet colour, &c. &c.

At the distance of five miles from Baltimore, in a north west direction, it assumes more the quality and appearance of French bur stone. At the distance of eleven or twelve miles, and near Randal's Town, it appears in considerable quantities above the surface, equally pure, and in every respect, calculated, apparently, to answer all the purposes of the best French bur stone. On each side of this range are a variety of substances, interesting to such as are fond of mineralogical pursuits, such as horne blend, horne stone, of various colours; likewise, the amianthinite, asbestinite, asbestoid common, &c. &c.

---

* As such, it is found in Saxony, near Kunmersdorf.

† See notes on Virginia.

Thus far I have endeavoured to give you the most correct information that the nature of my observations will permit, having never taken a single note on the different appearances that have presented themselves in the course of the frequent excursions which I have made in the vicinity of Baltimore.

In whatever I may have erred, it will, I hope, be corrected by some one, whose knowledge in the science is much more extensive, and whose disposition to cultivate and promote it, shall not only equal, but, very far surpass that of

Your obedient servant,

HORACE H. HAYDEN, S. D.

Dr. Nathaniel Potter.

……

Saunders on Gum Lac.

DEAR SIR,

As the natural history of many drugs introduced into the materia medica, remains in some obscurity, I suppose every information on the subject will be acceptable, and have therefore sent you an extract from the 79th volume of the Philosophical Transactions, by R. Saunders, M. D. of Boglepoor. W.

AS Lac is the produce of and a staple article of commerce in Assam, a country bordering on, and much connected with Thibet, some account of it may not be an improper supplement to the above remarks.* Lac is, strictly speaking, neither a gummy nor resinous substance, though it has some properties in common to both. Gums are soluble in water, and resins in spirits ; Lac admits of avery difficult union with either, without the mediation of some other agent. Lac is known in Europe by the different appellations of stick-lac, seed-lac and shell-lac. The first is the lac in pretty considerable lumps, with much of the woody parts of the branches of which it is formed adhering to it. Seed-lac is only the stick-lac broke into small pieces, garbed and appearing in a granulated form. Shell-lac is the purified lac, by a very simple process to be mentioned afterwards. Many vague and unauthenticated reports concerning lac, have reached the public; and though amongst the multiplicity of accounts, the true history of this substance has been nearly hit on, little credit is given in Europe to any description of it hitherto published. My observations, as far as they go, are the result of what I have seen, from the lack on the tree, the progress of the insect now in my custody, and the information of a gentleman residing at Goalpara, on the borders of Assam, who is perfectly conversant in the method of breeding the insect, inviting it to the tree, collecting the lac from the branches, and forming it into shell-lac, in which state much of it is received from Assam, and exported to Europe, for various great and useful purposes. The tree on which this fly most commonly generates, is known in Bengal by the name of the Biher-tree ,and is a species of the Rhamnus.* The fly is nourished by the tree, and there deposits its eggs, which nature has provided it with the means of defending from external injury by a collection of this lac, evidently serving the twofold purpose of a nidus and covering to the ovum, and insect in its first stage, and food for the maggot in its more advanced state. The lac is formed into complete cells, furnished with as much regularity and art as a honey-comb, but differently arranged. The flies are invited to deposit their eggs on the branches of the tree, by besmearing them with some of the fresh lac steeped in water, which attracts the fly, and gives a better and larger crop. The lac is collected twice a year, in the months of February and August. I have examined the egg of the fly with a very good micriscope: it is of a very pure red, perfectly transparent, except in the centre, where there were evident marks of the embryo forming, and opaque ramifications passing off from the body of it. The egg is perfectly oval, and about the size of an ant's egg. The maggot is about the one eighth of an inch long, formed of many rings (ten or twelve) with a small red head; when seen with a microscope, the parts of the head were easily distinguished, with six small specks on the breasts somewhat projected, which seemed to be the incipient formation of the feet. This maggot is now in my custody, in the form of a nymph or chrysalis, its annular coat forming a strong covering, from which it should issue forth a fly. I have never seen the fly, and cannot therefore describe it more fully, or determine its genus or species. I am promised a drawing of the insect in its different stages, and shall be able soon to add to a botanical description of the plant, a drawing of the branch with the different parts of fructification and lac on it. The gentleman to whom I owe part of my information, terms the lac the excrement of the insect. On a more minute investigation however, we may not find it more so than the wax or honey of the bee, or silk of the silk-worm. Nature has provided most insects with the means of secreting a substance, which generally answers the twofold purpose of defending the embryo, and supplying nourishment to the insect, from the time of its animation till able to wander about in quest of food. The fresh lac contains within its cells a liquid, sweetish to the taste, and of a fine red colour miscible in water. The natives of Assam use it as a dye, and cotton dipped in this liquid, makes afterwards a very good red ink. The simple operation of purifying lac is practised as follows. It is broken into small pieces, and picked from the branches and sticks when it is put into a sort of canvass bag, about four feet long, and not above six inches in circumference. Two of these bags are in constant use, and each of them held by two men. The bag is placed over a fire, and frequently turned, till the lac is liquid enough to pass through its pores, when it is taken off the fire, and squeezed by two men in different directions, dragging it along the convex part of a plantain-tree, prepared for the purpose; while this is doing, the other bag is heating to be treated in the same manner. The mucilaginous and smooth surface of the plantain-tree, seems peculiarly well adapted for preventing the adhesion of the heated lac, and giving it the form which enhances its value so much. The degree of pressure on the plantain-tree, regulates the thickness of the shell, and the quality of the bag determines its fineness and transparency. They have learned of late, that the lac which is thicker in the shell, than it used to be, is most prized in Europe. Assam furnishes us with the greatest quantity of lac in use; and it may not be generally known, that the tree on which they produce the best and largest quantity of lac is not uncommon in Bengal, and might be employed in propagating the fly, and cultivating the lac to great advantage. The small quantity of lac collected in these provinces, affords a precarious and uncertain crop, because not attended to. Some attention at particular seasons, is necessary to invite the fly to the tree; and collecting the whole of the lac with too great an avidity, where the insect is not very generally to be met with, may annihilate the breed. The best method of cultivating the tree, and preserving the insect, being understood in Bengal, would secure to the company's possessions the benefits arising from the sale of a lucrative article in great demand, and of extensive use.*

---

* Another paper, probably on the country of Thibet, by R. S.

---

* Buckthorn.

---

* The Lacca that comes to us from Judea in thin plates, is altogether resinous and completely soluble in alkohol, but it is very evident from the preceding account, that the unmanipulated stick-lac contains in its cells, a substance of an entire different quality, soluble in water. This matter is either the ovum or the pabulum. It is the only part used for dying, and for medicinal purposes; is highly praised for its astringent virtues when applied to the gums, as the Germans and Sweeds testify.

The prescription therefore in the Edinburgh Dispensatory (Tinet Lacca) is both unguarded and injudicious, for by it, nothing more than a varnish could be made, containing quite different qualities from these intended; and of course useless, perhaps we shall furnish some remarks hereafter on various other articles of the materia medica. W.

Potter's case of Luxated Humerus.

[As the Recorder in which this paper originally appeared, has long been discontinued, and the sphere of its circulation is therefore contracted, no other apology will be required for the republication of a case that has excited so much interest among the faculty.]

Frederick Siegfred Frede, the subject of the following report, a native of Prussia, sailed from Newcastle in England, on board the British brig Sylvan, Jonathan Ostle master, on the 26th of August, eighteen hundred and five, bound to Baltimore. On the seventh day after they lost the land, the wind blowing very high, and the brig being in a heavy sea, Frederick was ordered with others of his shipmates to hand the maintopsail. Having laid hold on the maintopsail sheet with his left hand, a sudden flaw filled the sail, and instantly backed it with such force, that he was obliged to hold on to save himself from being thrown from the yard. The exertion which this occasioned drew his arm upwards and backwards with great violence, and thereby threw the head of the *os humeri* completely under the *pectoral muscle.* The weather being uncommonly hard, and the winds adverse, the brig had a very tedious passage; twelve weeks had elapsed from the time she sailed from Newcastle before she made Cape Henry, and eleven from the day the accident occurred. During the passage Frederick continued unable to do duty, and after he arrived in port, two weeks had passed away, when his situation attracted the humane attention of their Danish and Swedish majesties consul, Mr. Collin. This gentleman equally remarkable for his humanity and the solicitude he manifests for the interests of the nations he represents, conducted the patient to my house and committed him to my care. He was a muscular, athletic man, about forty years old, but considerably emaciated by the pain and anxiety of mind occasioned by his disaster. Having collected a history of the case from the captain of the brig and the patient, I made a partial examination of the parts concerned, but such was the inflammation and tumefaction of all the parts from the cubitus to the scapula, that it was impossible to decide whether or not there was a dislocation, although from all the concomitant circumstances its existence was rendered very probable. The ordinary remedies for the subduction of inflammation were immediately prescribed and assiduously persevered in for two weeks. When the inflammation and swelling had so far subsided as to admit of a satisfactory examination, the real cause of all the patient's sufferings was so obvious that no time was to be lost in attempting (what I considered extremely problematical) a reduction. The ordinary modes of reducing a dislocated humerus were immediately employed, but as the inflammation of the parts interested had not entirely subsided, the sensibility of the parts rendered a sufficient extension impracticable. The patient was remanded to his lodgings, and enjoined to persevere in the use of the antiphlogistic regimen and remedies, and desired to return at the expiration of a week. He returned agreeably to appointment, and my efforts were repeated, but with the same result. Such was the rigidity of the muscles and I presume the strength of the newly formed adhesions, that they seemed to oppose an insuperable impediment to our views. After availing myself of the powers of three men, and using all the usual means that art had devised, I found that I had gained nothing towards the completion of my object. In this situation I informed Mr. Collin that I knew of only one remedy that promised to aid us in the reduction, and by our joint persuasions we prevailed on the patient to submit to blood letting *ad deliquium animi.* A vein was immediately opened, but upon reflecting on the quantity of blood, that would probably be necessary to lose, he suddenly determined to abandon this proceeding and I was compelled to stop the blood, without having attempted a reduction of the bone. I now expressed a determination to relinquish my attentions, and proposed a consultation, with a view of impressing the necessity of immediately proceeding to the last resort, in so forlorn a case. Doctor Cocke was immediately called in, and after hearing the preceeding history, concurred with me in opinion. The patient would not permit us to proceed to the operation, put procrastinated the time from day to day, till we lost nearly two weeks more, making in all, *seventeen weeks and three days* from the time he suffered the accident, till the day we adjusted the dislocated bone. Dr. Cocke and I had in this time repeated the ordinary means of reduction, and had remonstrated so strongly against the consequences that must attend him if the bone was permitted to remain in its then unnatural situation that he determined to surrender himself entirely to our discretion, and the next day was appointed for the attempt.

Our patient having seriously contemplated the gloomy prospect that presented itself, and finding that unless we succeeded he was probably destined to linger out the remnant of a miserable existence, in a hospital or poor-house, assumed a degree of resolution that enabled him to sustain the horrors of his situation with manly fortitude. Having already been so often baffled in my efforts in this case, and almost ready to despair of success, I surrendered the management of the patient to Dr. Cocke, and acted under his direction. Our apparatus was very simple. A strong towel was tightly rolled up, and formed into a ring sufficiently large to embrace the clavicle and scapula; the arm being passed through this ring, was fixed in such a situation that its upper part rested upon the middle of the clavicle while the under part laid near the inferior angle of the scapula; one corner of a sheet was attached to the forepart of the ring and the diagonal corner to the opposite point behind. Napkins were used in the usual way of making extension. Our apparatus being ready, we deemed it prudent to make one more effort before we opened a vein. Having placed three men on each side, they were desired to make the distention, which they did with great force, but with no other effect than an excruciating pain. The patient was now desired to stand erect, and a vein was immediately opened. The blood was permitted to flow till about three pounds had been drawn, when he fainted and sunk on the seat behind him; this was the signal for the re-application of the extending forces. In the minute during which the syncope lasted, Dr. Cocke placed his foot against the seat on which the patient was seated, and his knee under the axilla to elevate and direct the dislocated bone. The doctor added his strength to the powers already in action and the bone was restored to its natural situation with a loud crepitus occasioned no doubt by the rupture of the adhesions lately formed. From the length of time that had elapsed since the dislocation, and the noise that was audible by every person in the room, the adhesions must have been very strong to have required so powerful a force to lacerate them. The head of the bone had become so well accommodated to its new situation, that previous to the reduction the patient could lift a considerable weight, and move his arm backwards and forwards without pain. The muscles of the dislocated arm were considerably diminished in size and their power much impaired. He experienced a difficulty in extending the thumb and index, and nearly a month elapsed before the vigor of the parts was completely restored. Five weeks after the reduction, he sailed for Liverpool, and had two weeks before done his duty as an able bodied man in lading the ship and making other preparations for the voyage.

We are the more solicitous to preserve this case, because an opinion is almost universally entertained, that there is no possibility of reducing a bone at so late a period after its dislocation. Indeed it has been inculcated by many respectable surgeons, that no attempt should be made after a certain period, not more than half the time after which we have found it practicable to succeed. I cannot find an instance in the annals of the surgical art, where success has attended the efforts of the most eminent practitioners after the bone had been misplaced, nine, or even eight weeks, and the most dexterous and enterprizing of the present day have concurred in the presumed impracticability of such an operation. It is probable we are yet to learn how much the world is indebted to our countryman, Dr. Physick, for this admirable improvement. From the combination of unfortunate circumstances attending this case, it may be fairly presumed, that a dislocation under different circumstances might exist for a much longer time, and still the adhesions not have been so firmly formed. The situation, habits, and the utter destitution of surgical assistance concurred in permitting the inflammation of the parts concerned to run the course of nature; and consequently to become more perfectly formed than they could have been had the proper remedies to arrest the progress of inflammation been used immediately after the accident. I indulge an expectation, that the fortunate issue of this case will encourage others not to consider every long protracted case of dislocation as beyond the reach of art, and that it will encourage them to endeavour to imitate our example.

NATHANIEL POTTER.

……

WRIGHT,

On the Prussiate of Iron in Uterine Hemorrhage.

DEAR SIR,

AGREEABLY to promise I enclose for your consideration, some remarks on the use of the *Prussiate of Iron in Uterine Hemorrhage.*

In that action of the system which is the subject of this memoir, the theory of general disease which has fixed most firmly the attention of the medical world and established perhaps most permently the fame of its author, is, in my opinion exceedingly defective: I mean the hemorrhagie effort. No practitioner of medicine who has attended to a common epistaxis can admit the correctness of Dr. Brown's rejection of active hemorrhage. In offering a communication connected with this subject, I shall state facts with but few comments, and prefer no apology. I do not intend to entangle myself nor perplex others with a long drawn chain of causes and effects. The apparent astringent effects of particular articles taken into the stomach, is a string that I have never seen touched with much harmonious vibration. It is one of those points upon which I think physiology has reflected but few of her luminous rays. My observations upon this subject are purely practical, as they were at first simply experimental. They may be found to be nugatory, but, as medicine as it now is, is the result of experiment upon a rude and indigested mass of discordant materials and no arrangement among them, I see no harm in offering another subject for the ordeal.

Sometime in the summer of 1809, I was invited to a consultation by a very respectable member of the faculty in a case of protracted uterine hemorrhage after parturition. The subject was a lady of a delicate constitution, though she generally enjoyed a tolerable share of health. She had been delivered about two weeks previous and the labour was natural. The lochia came on early in rather more than the usual quantity, but not so profusely as to be productive of immediate danger or alarm. Instead, however, of declining in quantity and consistence as is usual, in the course of a few days it had rather increased and preserved its florid complexion. Under those circumstances great and increasing debility necessarily demanded every effort of restraint that art could suggest. Those efforts had been made as far as the usual routine of practice would permit; for, as far as I could judge, the treatment both as to medicine and regimen had been judicious. All the ordinary means resorted to in such cases had been employed, and had failed. The patient's extremities were cold; the pulse frequent and very small, and the general prostration of strength so great as to prevent her from speaking above a whisper.

In conferring with much solicitude on the situation of the patient, and reviewing the various modes of treatment that had been adopted in this disease, the attending physician mentioned to me a statement he had repeatedly received on this subject from an old lady in the neighbourhood, who had been forty years engaged in obstetric practice. She declared that it had been her practice, (and that it had been attended with the most flattering success,) to depend entirely in all such cases upon the administration of the substance known by the name of Prussian blue. As a woman, and not better informed than persons of her description generally are, she was incapable of making any distinctions in the states of her patients; but administered it as a specific, and she declared with almost uniform advantage. Her dose was half a teaspoonful in powder, repeated as she thought occasion seemed to require. The doctor informed me she was a woman of good character, and that he had heard from those whom she had attended, of many instances of her success by means of this remedy. Thus situated, with the case before us nearly hopeless, he intimated a desire to make trial of the medicine, and concluding that a doubtful remedy was better than none, I acquiesced.

I was under the necessity of leaving the patient immediately after the conversation, and did not see the doctor again for five or six months. The lady I understood had recovered, and when I next saw the doctor he informed me that he attributed her recovery to the use of the Prussiate, and mentioned the quantity she had taken, which I do not at this late period recollect. He informed me in addition, that he had subsequently treated several other cases of uterine hemorrhage in the same manner, and with invariable success.

This information from so respectable a source determined me to adopt with proper caution, the same course whenever I should meet with a case that baffled the usual remedies. Three cases only have since occurred to me, in which I found it necessary (or perhaps it would be better to say thought proper) to give the Prussiate of Iron. These were all within the last twelve months.

The first was a case in many respects similar to the one already detailed; except, that the discharge, at the time I saw the patient (ten days after her delivery) was rather a thin bloody mucus amounting to nearly a pound in twenty-four hours; but attended by all the debilitating effects of a discharge of the same quantity of red blood. She complained of no pain. With the regimen usual in such cases she took ten grains of the Prussiate,* in milk, every third hour. This was continued till she had taken ninety grains, at which time the discharge was diminished to a very trifling degree, and the pulse previously very small became fuller. Bark and Gum Kino were administered after the cessation of the discharge, and completed the cure.

The second case, was that of a pregnant negro woman, who about two months before her expected time sustained a violent shock from a fall which happened in climbing a fence. Before any person could get to her to raise her from the ground, she discharged from the uterus at least two pounds of blood. I happened to be present when this accident occurred, and seeing the discharge ordered her to be carried into the house immediately. She was too heavy to be taken up by the force around her, and as there was no time to be lost, they dragged her into the kitchen. The blood marked her passage to the house (which was a few yards from the place where she fell) in a continued stream. As soon as she reached the house she fainted. Her clothes were cut off immediately, her head supported, her hips raised while she laid on her back on the floor. In this situation cold water was kept continually pouring on her abdomen for an hour or more, to which time the discharge had been gradually decreasing.

---

* The choice of the Prussiate of iron is a consideration of primary importance. The Prussian blue of the shops is designated by numbers 1, 2, 3, and is pure in the inverse ratio of the numbers. No. 1, is the only quality that can with propriety be admitted into the materia medica, and from this the dose should be graduated. The matters with which the Prussic acid and iron are combined, are argillaceous earth and whiting. As these substances cannot contain the virtues ascribed to the Prussiate, they are to be viewed as adulterating commixtures. EDITOR.

She was now raised upon some blankets that laid near her, and cloths wet with cold vinegar and water were constantly applied to the abdomen and labia. This accident occurred in the afternoon and the plan prescribed was pursued through the night. On the following morning the discharge continued in the quantity of about the second or third day's lochia, without pain, and was pure blood. I now directed ten grains of the Prussiate in milk, and ordered it to be repeated in three hours. She took six doses at this interval, by which time the discharge entirely ceased.

I apprehended that as the discharge should cease, and reaction succeed its debilitating effect, fever and pain would come on, and premature labour take place. Nothing, however, of this kind happened. Uterogestation was carried to its full time and the patient had a good labour. She lost in the whole six pounds of blood.

The last case in which I used the Prussiate, occurred in the present month, and the patient is still under my care in a convalescent state. This case is not calculated to furnish arguments in favour of, or against the use of the medicine. In this case twelve or fourteen days after delivery the discharge was free, very debilitating and attended with slight irregular pains. I gave the Prussiate three days without benefit; at the end of which time a piece of membrane and a considerable portion of the placenta were discharged, and the symptoms disappeared.

Your obedient servant,

THOMAS H. WRIGHT.

Elkridge Landing, August 20, 1811.

Hermbstaedt's Experiments to obtain Sugar.

From the London Medical and Physical Journal, vol. IV.

Chemical Experiments and Observations, on the Extraction of Sugar and Syrup from Indigenous Plants. By SIGISM. HENRY HERMBSTAEDT.

FROM the chemical analysis of vegetable substances, and the knowledge of their constituent and other particles contained and mixed with them, it is sufficiently evident, that the East and West Indies are not the only countries provided by nature with saccharine plants, but saccharine matter is abundantly found in other productions of the vegetable kingdom; and it only requires an assiduous examination to point out those vegetables, from which it may be most copiously, and in the least expensive way, obtained.

Among the plants hitherto examined, none deserves to be ranked so near the true sugar-cane, as the whole genus of maple trees; and, particularly, the sugar and silver maple, *Acer saccharinum, and A. dasycarpon ehrh.* Both trees have been used for these fifty years to obtain sugar from,* which in the last eight years has proved to be exceedingly profitable.† By my own experiments which I have repeatedly made since the winter of 1796, I found out, that from all species of maples, sugar may be with more or less profit obtained, and that the sugar and silver maple growing even in Germany, though not in the best soil, give a very good raw sugar, not inferior to the best West India cane-sugar, and which is got so cheap, that a pound of it will come no higher than eighteen or twenty plennige, or about two-pence halfpenny; and only a groshen or a penny, when instead of charcoal, common coal or turf are employed for boiling the juice, and particularly when the operation is made upon a large scale, as one labourer is able to attend five hundred trees during the period of tapping them. The process of boiling the juice is, besides, so very simple, that any person may soon learn it. But these advantages are only to be expected from the sugar and silver maple, as the other species, *Acer negundo*, *A. campestre*, *A. Platanoides*, and A. pseudoplatanus contain a less quantity of sugar which is also not so rich in saccharine matter. However, as plantations of those maples require a space of twenty or twenty-five years before the trees are large enough to admit tapping, it will not be improper, but of great utility to the community, to examine in the mean while those indigenous plants, from which likewise a useful substitute for the West India sugar may be extracted; and it is with this view I have made the following experiments.

---

* An account of a kind of sugar made of the juice of maple in Canada; Philosophical Transactions, No. 171. Klam, in the Transactions of the Royal Swedish Acadamy of Sciences, year 1751. Memoirs sur les fucre d'Erable usite dans le Canada, &c. Hist. 1757.

† Notices sur l'Erable a sucre des etats unis, et sur les moyens d'en extraire le sucre, &c. par M. Rush, in Rozier's Observations sur la Physique, &c. Tom. xli. Paris, 1792.

1. *Experiments to obtain Sugar from India Corn.*

India corn, (zea mays) is said to contain, according to Vonsusti,[*] sugar, particularly in the nodes of the young stalks, from which Mr. Jacquin of Vienna,[†] has successfully prepared it; and this is farther confirmed by Mr. Marabelli[‡] in a dissertation on the subject. It is likewise reported, that the extraction of sugar from the stalks of India corn, growing particularly in marshy soils, has been tried in Italy upon a large scale, but afterwards left off again, as it was found not to answer the purpose, the sugar thus obtained being more expensive than common raw sugar. To be convinced by my own experience on this subject, I made some experiments, of which the following are the results: A quantity of India corn was cultivated in a tolerable and somewhat marshy soil, for the purpose. When the young plants were about six inches high, the leaves, when chewed, had a sweetish taste, but the stalks, particularly about the nodes, tasted like sugar. These young plants being cut off as near the ground as possible, freed from the leaves, and sufficiently cleaned, ten pounds of them were cut in pieces, and being pounded in a stone mortar, the juice was expressed, which weighed three pounds. This juice, whose sweetish taste had still a disagreeable flavour of herbs, was clarified with the white of eggs, after which that taste was scarcely perceptible, and being thickened to the consistence of a syrup, eight ounces of a very agreeable tasting syrup were obtained.

---

[*] Okonomische Schriften, i. e. Economical Writings, Tom. i. p. 307, and Tom. ii. p. 191.

[†] Crell's Chemical Annals, 1784, vol. I.

[‡] Franc. Marabelli de *Zea* Mays planta, analytica disquisitio. Pavia, 1793.

2. *Examination of the Spikes of India Corn.*

As the young spikes, when they are beginning to form, possess a very agreeable saccharine taste, they were thought fit for being examined. Ten pounds of them were accordingly squeezed in a stone mortar, and the juice expressed after the leaves had been stripped off. These gave four pounds of a milky juice, which could not be rendered perfectly clear by the white of eggs. By a slow evaporation, to the consistence of a syrup, nine ounces of an agreeable tasting brown syrup were got, but which differed from the former by being more mucilaginous.

3. *Examination of Stalks of India Corn of a more advanced growth.*

Twenty pounds of these stalks were cut in pieces, and, with the addition of water, squeezed in a stone mortar and the juice expressed, which possessed a disagreeable and somewhat acrid taste. Being in the same manner clarified, and thickened to the consistence of a syrup, twelve ounces of syrup were obtained, which had a disagreeable saline taste, and might rather be considered as a vegetable extract than as sugar.

4. *Experiments for obtaining dry Sugar from India Corn.*

To learn whether it was possible to exhibit a crystallized sugar from this plant, the syrups prepared from the young stalks and the spikes were each dissolved by itself in fresh lime-water, and gently boiled, by which a great part of their impurities was carried off. The liquors being strained through a woolen cloth, each of them was boiled to the thickness of a syrup, which was put in a glass, and set for eight months in a warm place, when little crystallizations of sugar appeared, which were with difficulty separated from the fluid. For this purpose, each syrup was evaporated by a gentle fire till it became dry, and this mass was digested with alkoholized spiritus vini to ebullition. The fluid, still hot, was instantly poured thro' some linen, whereon the mucilaginous parts remained; but on cooling the spiritous solution, a true sugar, of a yellow colour, crystallized in small grains. The alkohol being drawn from the remaining fluid by distillation, another portion of sugar was got by gentle evaporation, and altogether two ounces from the syrup of the young stalks, and one once and a half from that of the spikes.

By these experiments it is sufficiently shown, that from the young fresh stalks, as well as from the spikes of India corn, a true sugar can be extracted ; but as its separation from the gummy and other particles mixed with it, is attended with such difficulties, and the gain so very inconsiderable, that a pound of raw sugar from this plant, would cost one rix dollar, or above three shillings, it appears that no profit or economy will arise from the fabrication of this sugar.

5. *Experiments for obtaining Sugar from the Siberia Cow-Parsnep.*

The Russian cow-parsnep, (*Heracleum sphondylium Linn. Heracleum Sibericum*) has been long known as a plant containing a great deal of saccharine matter, in which respect, according to Steller, (in his travels to Kamtschatka, in German) it deserves the next place to the sugar cane, and the natives call it therefore *sweet-herb* or *katsh.* According to Gmelin (Flora Siberica, tom 1, p. 214) it does not differ from our common cow-parsnep; but others think it a particular species, to which they give the name of *sphondylium panaces.* The inhabitants of Kamtschatka gather the stalks of this plant in June, and having stripped off the leaves, they shave off the outer skin with muscle shells, and dry them in the sun, and afterwards they are chewed for the sake of sucking out the saccharine matter. In drying, the surface of the stalks is covered with white saccharine powder, which they separate by shaking them in a leather bag; but 40 lbs. of them afford only a quarter of a pound of this powder sugar, which therefore is considered as a great rarity. Besides this, the stalks and roots of the plants are employed for obtaining a sort of brandy.

I was supplied with some fresh plants of the Heracleum Sibericum for my experiments; but finding that the stalks were by no means so rich in sugar, as it is related of those plants growing in Siberia, I tried the roots, of which I got four pounds, the taste of which is sweetish like that of parsneps. Having freed them from the outer skin, they were dried, but no saccharine crust appeared on the surface; they were therefore ground, and being mixed with water, the juice was expressed, which tasted sweetish but a little acrid. Being boiled with the white of eggs, and clarified, it was thickened to the consistence of a syrup, of which six ounces were got, wherein, after a space of three months, a brown, grainy sugar had crystallized, which, however, was not quite free from a disagreeable flavour. Though it is shown by these experiments, that sugar may be obtained from that plant, yet the preparation of the sugar is too expensive for making use of it. It is, however, probable that the soil has a great influence upon the plant, and that therefore those growing in Siberia are richer in sugar.

6. *Experiments to obtain sugar from the Must of Wine.*

It might be presumed from the taste of must obtained from ripe grapes, that a considerable quantity of saccharine matter is contained in it, though involved by mucilage. To try whether a true sugar could be extracted from it, some experiments were undertaken. Eight Berlin quarts of must, from ripe sweet grapes, were seethed with the white of eggs, clarified, and filtered. The fluid being evaporated, gave three pounds of an agreeable but acidulous syrup. To take away this free acid, the syrup was dissolved again in lime water, and so much of it added, till no acid was perceived by reagency. The fluid being again clarified and evaporated, a very agreeable syrup was obtained, but from which it was by no means possible to procure crystallized sugar. However, this syrup would not be very profitable at the high price of must.

7. *Experiments with the Juice of the white and black Birch, for the sake of obtaining sugar.*

It has been much doubted, whether sugar might be got from the juice of those trees. Mr. Stæhlhammer, (Transactions of the Swedish Academy, vol. xxxv.) prepared from the juice of the white birch, *Betula alba*, tapped in the spring, a considerable quantity of syrup, superior to the common brown syrup, but not so good as that from the maple. Mr. Kalm (*Ibid.* vol. xiii.) relates, that from the North American black birch (*Betula nigra*, *carpinifolia*.) also called sugar birch, a great deal of sugar is obtained, which is not so sweet as that from the maple. Having received a sufficient quantity of the juice of both trees, I found that from one hundred and fifty Berlin quarts of the juice, extracted in April from fifty trees of the white birch, two pounds and a half of brown syrup were obtained, out of which no crystallized sugar could be got. But fifty quarts of the juice of the black birch gave one pound and a half of a very good and useful syrup, yet inferior to that of the maple. From this syrup a considerable portion of crystallized sugar separated; by which it appears, that sugar may be obtained from the black birch, but much inferior in quality to that of the maple, and at the same time, more expensive.

8. *Experiments to obtain Sugar from the Beet. (Beta eicla alba.)*

It is more than fifty years since the late Mr. Marggraff made several experiments with different species of the beet, to try how far it was possible to obtain a useful sugar and syrup from them; in which, however, he seems to have particularly succeeded with *Beta eicla*, as he extracted from two pounds of its fresh roots, half an once of good sugar. Induced by these experiments, I took sixty-eight pounds of the roots of beet, which being sufficiently cleaned, were ground and squeezed, and the residuum being moistened with warm water and again expressed, about twenty quarts of juice were obtained. A third part of this having evaporated, it was then mixed with twenty quarts of lime-water, and boiled for half an hour. The juice thus became clear, and of a yellowish colour, was filtered and afterwards thickened to the consistence of a syrup, of which six pounds were obtained in this way, of a brown yellowish colour, and an agreeable taste. A part of it was poured into an evaporating glass dish, in which some glass tubes had been placed; and being exposed for sixteen weeks in a warm situation, those tubes were found to be incrusted with crystals of sugar about the size of a pea, and very much like brown sugar-candy. A bushel of the beet roots costs about half a crown, and the expense of obtaining a syrup from them is not much above six-pence; a pound of the beet syrup would, therefore, come no higher than about four-pence, which is less than the price of the common syrup in Germany; consequently, it might prove a very advantageous substitute. The sugar may likewise be obtained cheap enough, in comparison to the present high price of the cane-sugar; but the operation to extract it is very long and slow; and though it equals the sugar of the maple in goodness, yet the process of extracting it is far more expensive.

9. *Experiments to obtain Sugar from another Species of Beet, (Beta vulgaris altissima, or Beta eicla altissima Jacquin. Bunkelrube, Germ.)*

This species of beet-root is commonly cultivated in some parts of Germany, as a very good food for cattle, and differs from the former by being more succulent and of a sweeter taste. The roots grow larger, and contain more juice, when cultivated in a rich soil. A bushel of these roots, which weighed about twelve pounds after the outer rind had been taken off, were ground upon a grater; and after a part of the juice had discharged itself during the grinding, the rest was expressed; by which means, twenty-four quarts of a very sweet violet-coloured juice were obtained. Taught by repeated experiments, I first boiled it to the evaporation of one third, by which a great deal of albuminous matter separated. When cool, it was poured through a woollen cloth mixed with twenty-four quarts of lime-water, and boiled for half an hour. The juice became in this way a clear fluid of a yellowish colour, being freed from a great part of its impurities. After having cooled and strained it, it was boiled to the thickness of a syrup, of which eight pounds were got. A part of this was likewise exposed in a deep evaporating dish, in which glass tubes were put, to a gentle evaporation, and after two months a deep yellow sugar was found to have crystallized on those tubes. As the crystallization of sugar proceeds very slowly in this syrup, as well as in the former, the quantity of sugar which it may contain, cannot be determined; but as this syrup is by far more agreeable than common syrup, and moreover, as including all expenses, a pound will cost no more than about three half-pence, it may likewise serve as a useful substitute. This species of beet is the same which Mr. Archard, of Berlin, has since used for the fabrication of sugar upon a larger scale.

10. *Experiments for obtaining Sugar from the Red Beet, (Betarubra, Beta eicla rubra, Jacquin.)*

The possibility of obtaining sugar from the common red beet has been already shown by Marggraff; I proceeded therefore in the same way as with the former, and obtained from a bushel of its roots six pounds and a half of syrup, which had rather an unpleasant rough taste, from which, however, it may perhaps be freed. I did not try to get sugar from it, though Marggraff obtained from eight ounces of dried roots, two and a half drachms of sugar.

(To be continued.)

……

From the Eclectic Repertory.

Inguinal Aneurism cured by tying the External Iliac Artery, in the Pelvis.

BY JOHN SYNG DORSEY, M. D.

ON the 15th of August, 1811, I was consulted by Alexander Patton, on account of a tumor in his right groin. The patient was a native of Aberdeenshire in Scotland, aged about thirty years, the last ten of which he had passed in America, having followed the trade of a cooper; was accustomed to hard labour, and to athletic exercise, jumping, running and the like. He was six feet in height, of a robust but not a corpulent habit.

Two years ago he perceived, for the first time, a small tumor in the right groin. Having never had the venereal, nor indeed any other disease, and not having met with any accident, he was at a loss to account for this appearance. From its commencement it throbbed with considerable violence. For a year and four months it encreased very slowly; during the last eight months much more rapidly. In January it was no bigger than a walnut; in August its shortest diameter was four inches, its longest, nearly five. It occasionally gave him severe pain, and at length incapacitated him from all labour. In June last he applied to Dr. Irwin, of Easton, the place of his residence, who instantly apprised him of the nature and importance of the complaint, and advised him to come to Philadelphia. He arrived here the 14th of August, and was admitted next day into the Pennsylvania hospital.

On examination, an aneurism was found, situated immediately below Poupart's ligament, forming a regular tumor in the groin, nearly hemispherical, with a kind of apex, where the skin appeared extremely thin, and discoloured as if by ecchymosis. The patient had used a good deal of exercise previously to his admission into the hospital, and had taken a drink of rum, in consequence of which his arterial system was greatly excited, and the tumor pulsated so violently that the bed clothes were bounced up with great force. He was confined to bed; was purged, and kept to a low diet. A consultation was called; and the surgeons of the house concurred in recommending the operation of tying the artery as high as practicable above the tumor. It was determined to perform the operation promptly; as the disease was progressing, and no benefit was to be expected from delay.

On Monday, 19th of August, at noon, in presence of Dr. Physick and Dr. Hartshorn, surgeons to the hospital, and a number of medical gentlemen, I proceeded to the operation. The patient, having previously taken fifty drops of laudanum, was placed on the table. An incision, three inches and a half long, was made, beginning an inch and a half higher than the superior anterior spinous process of the ilium, and one inch distant from that process internally; being also four inches and a half distant from the umbilicus, extending obliquely downward, and terminating about one inch above the basis of the tumor. This incision, which was nearly in the direction of the fibres of the tendon of the external oblique muscle, divided the skin and adipose membrane, and exposed that tendon, which was next cut through, the whole length of the external incision. The internal oblique muscle now protruded at the wound and was carefully cut through; the inferior edge of the transversalis abdominis was next divided, but not so far upward as the top of the external wound. My finger was then introduced, and the cellular texture readily yielded it a passage to the external iliac artery, the trunk of which I distinctly felt pulsating very strongly. With my finger I separated it gently from the neighbouring parts; but took care to denude only a very small portion of the vessel. The peritoneum I was equally careful to detach as little as possible; and not more than a square inch of it was disturbed. The only remaining difficulty in the operation was to pass the ligature round the vessel; and this having been anticipated, was readily surmounted. Before commencing the operation, I had secured an aneurismal needle (a blunt bodkin of silver properly bent) in a pair of curved forceps, by tying the handles of the forceps firmly together. The needle was armed with strong bobbin; and thus connected with the forceps, resembled a tenaculum, which could easily be managed outside of the wound. With one finger in the wound I found it very easy to direct the extremity of the needle, and with the forceps in my other hand, to push it through the fascia surrounding the vessel. The string connecting the handles of the forceps was now cut, and the needle was left under the vessel. The forceps being removed, the needle was drawn out, leaving the ligature round the artery. Convinced, by careful examination, that nothing but the artery was included in the ligature, and that it was, to the best of my judgment, natural in size and texture, I tied it very firmly, as high up as possible. The pulsation of the tumor instantly ceased. Three knots were made, and the ends of the ligature were left out at the external wound. No blood vessel of magnitude was divided, and not half an ounce of blood was lost. No stitches were employed to close the wound; a strip of adhesive plaster effectually answered this purpose. A pledget of lint was applied, and the patient was put to bed, his thigh being moderately flexed upon the pelvis. He complained of extreme pain during the latter part of the operation, the whole of which occupied eleven minutes.

The patient's pulse, for several days before the operation, was 80: after the operation it was 88, and rose in the afternoon to 100. At four o'clock he was bled ten ounces. At seven he complained of extreme pain in the back and belly, and also of some pain in the limb. He was not permitted to take any sustenance except toast and water. The superficial veins of the leg and foot were filled; and the whole of the limb was covered all the evening with perspiration. Its temperature was examined repeatedly by a thermometer, and was five degrees colder than the other. It was covered with flannel and carded wool.

Tuesday 20th. Passed a restless night, in great pain. To use his own language, in expressing his sensations, "he felt as if his loins were tearing apart." He was also troubled with pain of the bowels. Three grains of calomel and ten of rhubarb were given, but without procuring a stool. In the afternoon he was bled ten ounces, and a purgative injection was ordered; after which his bowels were freely opened, and his pain subsided. An enema, consisting of a hundred drops of laudanum and two ounces of water, was administered, and he soon after fell asleep. The weather, on the day of operation and several days after, was very hot. The mercury of the thermometer in the patient's room stood at 86° Fahrenheit. Placed between the toes of the aneurismal limb, it rose to 88°; between those of the sound limb 90°, at both knees it stood at 92°. His pulse was 100, and tense.

Wednesday 21st, *third day* after the operation. The sleep procured by the anodyne injection continued all night. In the fore part of the day he was easy: but in the evening his pain returned with considerable fever. He was bled ten ounces and took ten grains of magnesia and as much rhubarb: this with the assistance of a clyster, brought away a large quantity of feces and flatus, and procured relief of all his pain. The anodyne injection was again administered, and he soon after slept. His pulse 100 and somewhat tense.

Thursday, *fourth day*. He slept all night and was much better; being quite free from pain and fever. His pulse 90. He eat some boiled rice with great relish. The wound was examined and it was found that nearly all of it had united: a little healthy pus surrounded the ligature. The limb was four degrees colder than the sound one.

From this time no change of importance occurred until Sunday, 1st Sept. when the ligature came away; viz. on the fourteenth day after the operation. In a few days more the wound cicatrized, without the occurrence of a single unpleasant symptom. On the twentieth day after the operation, his nurse being absent, he arose from the bed and walked across the room, and has taken exercise every day since without inconvenience. The tumor in the groin diminishes slowly, and at this time is very much reduced in size.

Remarks. The operation of tying up the external iliac artery above Pourpart's ligament was first performed by Mr. Abernethy, under circumstances in which immediate death was the only alternative. He repeated it afterwards in cases of aneurism seated so high in the femoral artery as to preclude all prospect of a cure by any other means. He performed the operation four times. In the first two instances his patients died; in the succeeding cases they recovered[*] Mr. Freer, in the Birmingham hospital, performed the operation in a case of inguinal aneurism with complete success; and soon after another case was treated successfully by Mr. Tomlinson, in the same hospital.[†] These six cases are all I have seen related, in which the operation has been tried. The case I have now detailed is the seventh; and it has failed in only two of these. In every instance the limb has been supplied with blood, which does not uniformly happen after the operation for popliteal aneurism.

I wish, before closing this paper to call the attention of those surgeons, who may have occasion to perform the operation, to the forceps, an engraving of which is annexed[‡]. This instrument was contrived several years ago by Dr. Physick for the purpose of passing a needle under the pudic artery, when wounded in lithotomy, and has since been used by him for securing bleeding arteries in deep narrow wounds. Mr. Abernethy complains of "the great difficulty of turning a common needle in a deep narrow wound;" and Mr. Freer

[*] Abernethy's Surgical Observation.

[†] Freer on Aneurism.

[‡] To the original paper in the Electric Repertory.

was unable to pass his aneurismal needle round the iliac artery, until he punctured the fascia surrounding it with his knife, which he confessed was the most " difficult and dangerous part of the operation." These dangers and difficulties are entirely obviated by means of the curved forceps; and I think the operation greatly simplified by the use of this contrivance.

Should this paper meet the eyes of Mr. Abernethy, I hope he will be gratified with the additional testimony of the importance of an operation for which the world is indebted to the intrepid efforts of true genius; and he will no doubt learn with pleasure, that one individual on this side the Atlantic owes to it, his life.

Arch Street, 20*th September*, 1811.'

AN ACT

To establish and incorporate a Medical and Chirurgical Faculty or Society, in the State of Maryland—passed 1798.

WHEREAS it appears to the General Assembly of Maryland, that the establishment and incorporation of a Medical and Chirurgical Faculty or Society of Physicians and Surgeons, in the said state, will be attended with the most beneficial and salutary consequences, by promoting and disseminating medical and chirurgical knowledge throughout the state, and may in future prevent the citizens thereof from risking their lives in the hands of ignorant practitioners or pretenders to the healing art—Therefore,

II. *Be it enacted by the General Assembly of Maryland,* That Gustavus Brown, William Lansdale, Barton Tabbs, Elijah Jackson and William H. Roach, of St. Mary's county; James M. Anderson, junr. Morgan Brown, junr. Edward Scott, Robert Geddes and Edward Worrel, of Kent county; Charles Alexander Warfield, Richard Hopkins, Wilson Waters, Thomas Noble Stocket and William Murray, of Ann-Arundel county; Thomas Bourne, Thomas Parran, Joseph Ireland, Daniel Rawlings and James Gray, of Calvert county; John Parnham, Gustavus Richard Brown, Daniel Jenifer and Gerhard Wood, of Charles county; Thomas Cradock, Thomas Love, John Cromwell, Philip Trapnel and Christopher Todd, of Baltimore county; Perry E. Noel, Stephen Theodore Johnson, Tristram Thomas and Ennalls Martin, of Talbot county; Levin Irvin, Arnold Elsey, Ezekiel Haynie, John Woolford and Mathias Jones, of Somerset county; Edward White, James Sullivane, Dorsey Wyvill, William Hays and Howes Goldsborough, of Dorchester county; Abraham Mitchell, William Miller, Elisha Harrison, John Groome and John King, of Cæcil county; Richard I. Duckett, William Beans, junr. William Marshall, William Baker and Kobert Pottenger, of Prince

George's county; Upton Scott, James Murray, John Thomas Shaaff and Riverdy Gheselin, of the city of Annapolis; James Davidson, John Wells, Samuel Thompson, Robert Goldsborough and John Thomas, of Queen Anne's county; John Neille, Thomas Fossett, George Washington Purnell, John Purnel and John Huston, of Worchester county, Philip Thomas, Francis Brown Sappington, William Hyllary, John Tyler and Joseph Sim Smith, of Frederick county; John Archer, Thomas H. Birckhead, Elijah Davis and Thomas Archer, of Harford county; Jesse Downes, John Young, junr. William B. Keene, Joseph Price and Henry Helm, of Caroline county; George Buchanan, Lyde Goodwin, Ashton Alexander, Arthur Pue, Daniel Moores and Henry Stevenson, of the city of Baltimore; Richard Pindell, Samuel Young, Peter Waltz, Jacob Schnively and Zachariah Clagett, of Washington county; Edward Gantt, Charles Worthington, Joseph Hall, Zadock Magruder, junr. James Anderson and Charles A. Beatty, of Montgomery county; Benjamin Murrow, James Forbes and George Lynn, of Allegany county, and such persons as they may from time to time elect, and their successors, are hereby declared to be one community, corporation and body politic, forever, by and under the name and title of the *Medical and Chirurgical Faculty* of the state of Maryland, and by and under the same name and title they shall be able and capable in law to purchase, take, have and enjoy to them and their successors, in fee, or for lease estate or estates, any lands, tenements, rents, annuities, chattels, bank stock, registered debts, or other public securities within this state, by the gift, bargain, sale or devise of any person or persons, bodies politic or corporate capable to make the same, and the same at their pleasure to alien, sell, transfer or lease, and apply to such purposes as they may adjudge most conducive to the promoting and disseminating medical and surgical knowledge, or to alleviating the calamities and miseries of their fellow-citizens: Provided, nevertheless, that the said faculty or body politic shall not, at any one time, hold or possess property, real, personal or mixed, exceeding in total value the sum of ten thousand dollars per annum.

III. *And be it enacted*, That the members of the said faculty above designated, may and shall hold their first meeting, at the city of Annapolis, on the first Monday in June next or as soon thereafter as a number, not less than fifteen of them, can be convened; at which meeting they may appoint a president, a secretary and treasurer, make a common seal, and may elect into their body such medical and chirurgical practitioners within this state, as they may deem qualified to become members of the faculty.

IV. *And be it enacted*, That it shall and may be lawful for the said medical faculty, or any number of them then attending, (not less than fifteen) to elect by ballot twelve persons of the greatest chirurgical abilities in the state, who shall be styled the *Medical Board of Examiners* for the state of Maryland, seven of whom shall be residents of the Western and five of the Eastern Shore of Maryland, whose duty it shall be to grant licenses to such medical and chirurgical gentlemen as they, either upon a full examination, or upon the production of diplomas from some respectable college, may judge adequate to commence the practice of the medical and chirurgical arts, each person so obtaining a certificate to pay a sum not exceeding ten dollars, to be fixed on or ascertained by the faculty.

V. *And be it enacted*, That any five of the examiners appointed for the Western, and any three of those appointed for the Eastern Shore, shall constitute a board on their respective shores for examining such candidates as may apply on the said shores respectively, and shall subscribe their names to each certificate by them granted, which certificate shall be also countersigned by the president of the faculty, and have the seal of the faculty affixed thereto by the secretary upon the payment into the hands of the treasurer, the sum of money to be ascertained as above, by the faculty; and any one of the said examiners may grant a license to practice until a board, in conformity of this act, can be held.

VI. *And be it enacted*, That after the appointment of the aforesaid Medical Board, no person, not already a practitioner of medicine or surgery, shall be allowed to practice in either of the said branches and receive payment for his services, without first having obtained a license, certified as by this law directed, under the penalty of fifty dollars for each offence, to be recovered in the county court where he may reside, by bill of presentment and indictment, one half for the use of the faculty, and the other for that of the informer.

VII. *And be it enacted*, That every person, who upon application, shall be elected a member of the Medical Faculty, shall pay a sum not exceeding ten dollars, to be ascertained by the faculty.

VIII. *And be it enacted*, That the Medical Faculty be and they are hereby empowered, from time to time, to make such bye-laws, rules and regulations, as they may find requisite, to break or alter their common seal, to fix the times and places for their general meetings, for the meetings of the Board of Examiners, the modes and times of electing officers, filling up vacancies in the Medical Board, and to do and perform such other things as may be requisite for carrying this act into execution, and which may not be repugnant to the constitution and laws of this State or the United States.

Supplement, passed 1801.

No person who is not already a practitioner of medicine or surgery, within this state, shall be allowed to practice in either of the said branches, and recieve payment for the same, without having first obtained a license agreeably to the original act to which this is a supplement, under the penalty of fifty dollars for each offence, to be recovered in the county court where the offence is committed, by presentment and bill of indictment ; one half for the use of the faculty, and the other for the informer.

Nothing herein contained shall extend, or be construed to extend to prohibit any person, during his actual residence in any other of the United States, and who by the laws of the state where he doth or may reside, is not prohibited from practising in either of the above branches, from practising in this state, within fifteen miles from the residence of such practitioner: Provided always, that it shall and may be lawful for any person resident as aforesaid, and not prohibited as aforesaid, when specially sent for to come into any part of this state, and administer or prescribe medicine, or perform any kind of surgical operation for the relief of such to whose assistance he may be sent for.

THE

BALTIMORE

Medical and Philosophical Lycæum.

Vol. I,—No. IV.

FOR OCTOBER, NOVEMBER, & DECEMBER, 1811.

TO DR. POTTER.

DEAR SIR,

I received inclosed in a letter, a few days since from my friend Mr. Chrales Bell, of London, an essay on the anatomy, and physiology of some particular parts of the brain. As this is in all probability, the only copy which has yet reached the United States, it may be gratifying to the faculty to become acquainted as early as possible with the author's views. You are at liberty therefore (if you think proper) to give it a place in your journal. Yours, &c.

WILLIAM GIBSON.

Baltimore, *March*, 13*th* 1812.

NOTE.

THE want of any consistent history of the Brain and Nerves, and the dull unmeaning manner which is in use of demonstrating the brain, may authorize any novelty in the manner of treating the subject.

I have found some of my friends so mistaken in their conception of the object of the demonstrations which I have delivered in my lectures, that I wish to vindicate myself at all hazards. They would have it that I am in search of the seat of the soul; but I wish only to investigate the structure of the brain, as we examine the structure of the eye and ear.

It is not more presumptuous to follow the tracts of nervous matter in the brain, and to attempt to discover the course of sensation, than it is to trace the rays of light through the humours of the eye, and to say, that the retina is the seat of vision. Why are we to close the investigation with the discovery of the external organ?

It would have been easy to have given this essay an imposing splendour, by illustrations and engravings of the parts, but I submit it as a sketch to those who are well able to judge of it in this shape.

The prevailing doctrine of the anatomical schools, is, that the whole brain is a common sensorium; that the extremities of the nerves are organized, so that each is fitted to receive a peculiar impression; or that they are distinguished from each other only by delicacy of structure, and by a corresponding delicacy of sensation; that the nerve of the eye, for example, differs from the nerves of touch only in the degree of its sensibility.

It is imagined that impressions, thus differing in kind, are carried along the nerves to the sensorium, and presented to the mind; and that the mind, by the same nerves which receive sensations, sends out the mandate of the will to the moving parts of the body.

It is further imagined, that there is a set of nerves, called vital nerves, which are less strictly connected with the sensorium, or which have upon them knots, cutting off the course of sensation and thereby excluding the vital motions from the government of the will.

This appears sufficiently simple and consistent, until we begin to examine anatomically the structure of the brain, and the course of the nerves,—then all is confusion; the divisions and subdivisions of the brain, the circuitous course of the nerves, their intricate connections, their separation and re-union, are puzzling in the last degree, and are indeed considered as things inscrutable. Thus it is, that he who knows the parts the best, is most in a maze, and he who knows least of anatomy, sees least inconsistency in the commonly received opinion.

In opposition to these opinions, I have to offer reasons for believing—That the cerebrum and cerebellum are different in function as in form; That the parts of the cerebrum have different functions; and that the nerves which we trace in the body are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office, as they are in origin, from the brain.

That the external organs of the senses have the matter of the nerves adapted to receive certain impressions, while the corresponding organs of the brain are put in activity by the external excitement: That the idea or perception is according to the part of the brain to which the nerve is attached, and that each organ has a certain limited number of changes to be wrought upon it by the external impression:

That the nerves of sense, the nerves of motion, and the vital nerves, are distinct through their whole course, though they seem sometimes united in one bundle; and that they depend for their attributes on the organs of the brain to which they are severally attached.

The view which I have to present, will serve to shew why there are divisions, and manydistinct parts in the brain: why some nerves are simple in their origin and distribution, and others intricate beyond description. It will explain the apparently accidental connection betwen the twigs of nerves. It will do away the difficulty of conceiving how sensation and volition should be the operation of the same nerve at the same moment. It will shew how a nerve may lose one property, and retain another; and it will give an interest to the labours of the anatomist in tracing the nerves.

Idea of a new Anatomy of the Brain; submitted for the observations of his friends; by Charles Bell, F.R.S.E.

WHEN, in contemplating the structure of the eye, we say, how admirably it is adapted to the laws of light we use language which implies a partial, and consequently an erroneous view. And the philosopher takes not a more enlarged survey of nature when he declares how curiously the laws of light are adapted to the constitution of the eye.

This creation, of which we are a part, has not been formed in parts. The organ of vision, and the matter or influence carried to the organ, and the qualities of bodies with which we are acquainted through it, are parts of a system great beyond our imperfect comprehension, formed as it should seem at once in wisdom; not pieced together like the work of human ingenuity.

When this whole was created, (of which the remote planetary system, as well as our bodies, and the objects more familiar to our observation, are but parts) the mind was placed in a body not merely suited to its residence, but in circumstances to be moved by the materials around it; and the capacities of the mind, and the powers of the organs, which are as a medium betwixt the mind and the external world, have an original constitution framed in relation to the qualities of things.

It is admitted that neither bodies nor the images of bodies enter the brain. It is indeed imposible to believe that colour can be conveyed along a nerve; or the vibration in which we suppose sound to consist can be retained in the brain: but we can conceive, and have reason to believe, that an impression is made upon the organs of the outward senses when we see, or hear, or taste.

In this inquiry it is most essential to observe, that while each organ of sense is provided with a capacity of receiving certain changes to be played upon it, as it were, yet each is utterly incapable of receiving the impressions destined for another organ of sensation.

It is also very remarkable that an impression made on two different nerves of sense, though with the same instrument, will produce two distinct sensations; and the ideas resulting will only have relation to the organ affected.

As the announcing of these facts forms a natural introduction to the Anatomy of the Brain, which I am about to deliver, I shall state them more fully.

There are four kinds of Papillæ on the tongue, but with two of those only we have to do at present. Of these, the Papillæ of one kind form the seat of the sense of taste; the other Papillæ (more numerous and smaller) resemble the extremities of the nerves in the common skin, and are the organs of touch in the tongue. When I take a sharp steel point, and touch one of *these* Papillæ, I feel the sharpness. The sense of *touch* informs me of the shape of the instrument. When I touch a Papilla of taste, I have no sensation similar to the former. I do not know that a point touches the tongue, but I am sensible of a metallic taste, and the sensation passes backward on the tongue.

In the operation of couching the cataract, the pain of piercing the retina with a needle is not so great as that which proceeds from a grain of sand under the eyelid. And although the derangement of the stomach sometimes marks the injury of an organ so delicate, yet the pain is occasioned by piercing the outward coat, not by the affection of the expanded nerve of vision.

If the sensation of light were conveyed to us by the retina, the organ of vision, in consequence of that organ being as much more sensible than the surface of the body as the impression of light is more delicate than that pressure which gives us the sense of touch; what would be the feelings of a man subjected to an operation in which a needle were pushed through the nerve? Life could not bear so great a pain.

But there is an occurence during this operation on the eye which will direct us to the truth; when the needle pierces the eye, the patient has the sensation of a spark of fire before the eye.

This fact is corroborated by experiments made on the eye. When the eye-ball is pressed on the side, we perceive various coloured light. Indeed the mere effect of a blow on the head might inform us, that sensation depends on the exercise of the organ affected, not on the impression conveyed to the external organ; for by the vibration caused hy the blow, the ears ring, and the eye flashes light, while there is neither light, nor sound present.

It may be said, that there is here no proof of the sensation being in the brain more than in the external organ of sense. But when the nerve of a stump is touched, the pain is as if in the amputated extremity. If it be still said that this is no proper example of a peculiar sense existing without its external organ, I offer the follow example: Quando penis glandem exedat ulcus, et nihil nisi granulatio maneat, ad extremam tamen nervi pudicæ partem ubi terminatur sensus supersunt, et exquisitissima sensus gratificatio.

If light, pressure, galvanism, or electricity produce vision, we must conclude that the idea in the mind is the result of an action excited in the eye or in the brain, not of any thing received, though caused by an impression from without. The operations of the mind are confined not by the limited nature of things created, but by the limited number of our organs of sense. By induction we know that things exist which yet are not brought under the operation of the senses. When we have never known the operation of one of the organs of the five senses, we can never know the ideas pertaining to that sense; and what would be the effect on our minds, even constituted as they now are, with a surperadded organ of sense, no man can distinctly imagine.

As we are parts of the creation, so God has bound us to the material world by this law of our nature, that it shall require excitement from without, and an operation produced by the action of things external to rouse our faculties: But that once brought into activity, the organs can be put in exercise by the mind, and be made to minister to the memory and imagination, and all the faculties of the soul.

I shall hereafter shew, that the operations of the mind are seated in the great mass of the cerebrum, while the parts of the brain to which the nerves of sense tend, strictly form the seat of the sensation; being the internal organs of sense. These organs are operated upon in two directions. They receive the impression from without, as from the eye and ear: and as their action influences the operations of the brain producing perception, so are they brought into action and suffer changes similar to that which they experience from external pressure by the operation of the will; or, as I am now treating of the subject anatomically, by the operation of the great mass of the brain upon them.

In all regulated actions of the muscles we must acknowledge that they are influenced through the same nerves, by the same operation of the sensorium. Now the operations of the body are as nice and curious, and as perfectly regulated before reason has sway, as they are at any time after, when the muscular frame might be supposed to be under the guidance of sense and reason. Instinctive motions are the operations of the same organs, the brain and nerves and muscles, which minister to reason and volition in our mature years. When the young of any animal turns to the nipple, directed by the sense of smelling, the same operations are performed, and through the same means, as afterwards when we make an effort to avoid what is noxious, or desire and move towards what is agreeable.

The operations of the brain may be said to be three fold: 1. The frame of the body is endowed with the characters of life, and the vital parts held together as one system through the operation of the brain and nerves; and the secret operations of the vital organs suffer the controul of the brain, though we are unconscious of the thousand delicate operations which are every instant going on in the body. 2. In the second place, the instinctive motions which precede the development of the intellectual faculties are performed through the brain and nerves. 3. In the last place, the operation of the senses in rouzing the faculties of the mind and the exercise of the mind over the moving parts of the body, is through the brain and nerves. The first of these is perfect in nature, and independent of the mind. The second is a prescribed and limited operation of the instrument of thought and agency. The last begins by imperceptible degrees, and has no limit in extent and variety. It is that to which all the rest is subservient, the end being the calling into activity and the sustaining of an intellectual being.

Thus we see that in as far as is necessary to the great system, the operation of the brain, nerves, and muscles are perfect from the beginning; and we are naturally moved to ask, Might not the operations of the mind have been thus perfect and spontaneous from the beginning as well as slowly excited into action by outward impressions? Then man would have been an insulated being, not only cut off from the inanimate world around him, but from his fellows; he would have been an individual, not a part of a whole. That he may have a motive and a spring to action, and suffer pain and pleasure, and become an intelligent being, answerable for his actions,—sensation is made to result from external impressions, and reason and passion to come from the experience of good and evil; first as they are in reference to his corporeal frame, and finally as they belong to the intellectual privations and enjoyments.

The brain is a mass of soft matter, in part of a white colour, and generally striated; in part of a grey or cineritious colour, having no fibrous appearance. It has grand divisions and subdivisions: and as the forms exist before the solid bone incloses the brain; and as the distinctions of parts are equally observable in animals whose brain is surrounded with fluid, they evidently are not accidental, but are a consequence of internal structure; or in other words they have a correspondence with distinctions in the uses of the parts of the brain.

On examining the grand divisions of the brain we are forced to admit that there are four brains. For the brain is divided longitudinally by a deep fissure; and the line of distinction can even be traced where the sides are united in substance. Whatever we observe on one side has a corresponding part on the other; and an exact resemblance and symmetry is preserved in all the lateral divisions of the brain. And so, if we take the proof of anatomy, we must admit that as the nerves are double, and the organs of sense double, so is the brain double; and every sensation conveyed to the brain is conveyed to the two lateral parts; and the operations performed must be done in both lateral portions at the same moment.

I speak of the lateral divisions of the brain being distinct brains combined in function, in order the more strongly to mark the distinction betwixt the anterior and posterior grand divisions. Betwixt the lateral parts there is a strict resemblance in form and substance: each principal part is united by transverse tracts of medullary matter; and there is every provision for their acting with perfect sympathy. On the contrary, the *cerebrum*, the anterior grand division, and the *cerebellum* the posterior grand division, have slight and indirect connexion. In form and division of parts, arrangement of white and grey matter, there is no resemblance. There is here nothing of that symmetry and correspondence of parts which is so remarkable betwixt the right and left portions.

I have found evidence that the vascular system of the cerebellum may be affected independently of the vessels of the cerebrum. I have seen the whole surface of the cerebellum studded with spots of extravasated blood as small as pin heads, so as to be quite red, while no mark of disease was upon the surface of the cerebrum. The action of vessels it is needless to say is under the influence of the parts to which they go; and in this we have a proof of a distinct state of activity in the cerebrum and cerebellum.

From these facts, were there no others, we are entitled to conclude, that in the operations excited in the brain there cannot be such sympathy or corresponding movement in the cerebrum and cerebellum as there is betwixt the lateral portions of the cerebrum; that the anterior and posterior grand divisions of the brain perform distinct offices.

In examining this subject further, we find, when we compare the relative magnitude of the cerebrum to the other parts of the brain in man and in brutes, that in the latter the cerebrum is much smaller, having nothing of the relative magnitude and importance which in man it bears to the other parts of the nervous system; signifying that the cerebrum is the seat of those qualities of mind which distinguish man. We may observe also that the posterior grand division, or *cerebellum* remains more permanent in form; while the cerebrum changes in conformity to the organs of sense, or the endowment of the different classes of animals. In the inferior animals, for example, where there are two external organs of the same sense, there is to be found two distinct corresponding portions of cerebrum, while the cerebellum corresponds with the frame of the body.

In thinking of this subject, it is natural to expect that we should be able to put the matter to proof by experiment. But how is this to be accomplished, since any experiment direct upon the brain itself must be difficult, if not impossible?—I took this view of the subject. The *medulla spinalis* has a central division, and also a distinction into anterior and posterior fasciculi, corresponding with the anterior and posterior portions of the brain. Further we can trace down the crura of the *cerebrum* into the anterior fasciculus of the spinal marrow and the crura of the *cerebellum* into the posterior fasciculus. I thought that here I might have an opportunity of touching the *cerebellum*, as it were, through the posterior portion of the spinal marrow, and the cerebrum by the anterior portion. To this end I made experiments which, though they were not conclusive, encouraged me in the view I had taken.

I found that injury done to the anterior portion of the spinal marrow, convulsed the animal more certainly than injury done to the posterior portion; but I found it difficult to make the experiment without injuring both portions.

Next considering that the spinal nerves have a double root and being of opinion that the properties of the nerves are derived from their connections with the parts of the brain, I thought that I had an opportunity of putting my opinion to the test of experiment, and of proving at the same time that nerves of different endowments were in the same cord, and held together by the same sheath.

On laying bare the root of the spinal nerves, I found that I could cut across the posterior fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed.

Such were my reasons for concluding that the cerebrum and the cerebellum were parts distinct in function, and that every nerve possessing a double function obtained that by having a double root. I now saw the meaning of the double connexion of the nerves with the spinal marrow; and also the cause of that seeming intricacy in the connexions of nerves throughout their course, which were not double at their origins.

The spinal nerves being double, and having their roots in the spinal marrow, of which a portion comes from the cerebrum and a portion from the cerebellum, they convey the attributes of both grand divisions of the brain to every part; and therefore the distribution of such nerves is simple, one nerve supplying its destined part. But the nerves which come directly from the brain, come from parts of the brain which vary in operation; and in order to bestow different qualities on the parts to which the nerves are distributed, two or more nerves must be united in their course, or at their final destination. Hence it is that the first nerve must have branches of the fifth united with it: hence the *portio dura* of the seventh pervades every where the bones of the cranium to unite with the extended branches of the fifth: hence the union of the third and fifth in the orbit: hence the ninth and fifth are both sent to the tongue: hence it is, in short, that no part is sufficiently supplied by one single nerve, unless that nerve be a nerve of the spinal marrow, and have a double root, a connexion (however remotely) with both the cerebrum and cerebellum.

Such nerves as are single in their origin from the spinal marrow will be found either to unite in their course with some other nerve, or to be such as are acknowledged to be peculiar in their operation.

The eighth nerve is from the portion of the *medulla oblongata** which belongs to the cerebellum : the ninth nerve comes from the portion which belongs to the cerebrum. The first is a nerve of the class called Vital nerves, controuling secretly the operation of the body; the last is the Motor nerve of the tongue, and is an instrument of volition. Now the connexions formed by the eighth nerve in its course to the viscera are endless; it seems no where sufficient for the entire purpose of a nerve; for every where it is accompanied by others, and the ninth passes to the tongue, which is already profusely supplied by the fifth.

---

* The *medulla oblongata* is only the commencement of the spinal marrow.

Understanding the origin of the nerves in the brain to be the source of their powers, we look upon the connexions formed betwixt distant nerves, and upon the combination of nerves in their passage, with some interest; but without this the whole is an unmeaning tissue. Seeing the seeming irregularity in one subject, we say it is accident; but finding that the connexions never vary, we say only that it is strange, until we come to understand the necessity of nerves being combined in order to bestow distinct qualities on the parts to which they are sent.

The *cerebellum* when compared with the *cerebrum* is simple in its form. It has no internal tubercles or masses of cineritious matter in it. The medullary matter comes down from the cineritious cortex, and forms the *crus;* and the *crus* runs into union with the same process from the cerebrum; and they together form the *medulla spinalis*, and are continued down into the spinal marrow; and these crura or processes afford double origin to the double nerves of the spine. The nerves proceeding from the Crus Cerebelli go every where (in seeming union with those from the Crus Cerebri); they unite the body together, and controul the actions of the bodily frame; and especially govern the operation of the viscera necessary to the continuance of life.

In all animals having a nervous system, the *cerebellum* is apparent, even though there be no *cerebrum*. The cerebrum is seen in such tribes of animals as have organs of sense, and it is seen to be near the eyes, or principal organ of sense; and sometimes it is quite separate from the *cerebellum*.

The cerebrum I consider as the grand organ by which the mind is united to the body. Into it all the nerves from the external organs of the senses enter; and from it all the nerves which are agents of the will pass out.

If this be not at once obvious, it proceeds only from the circumstance that the nerves take their origin from the different parts of the brain; and while those nerves are considered as simple cords, this circumstance stands opposed to the conclusion which otherways would be drawn. A nerve having several roots, implies that it propagates its sensation, to the brain generally. But when we find that the several roots are distinct in their endowments, and are in respect to office distinct nerves; then the conclusion is unavoidable, that the portions of the brain are distinct organs of different functions.

To arrive at any understanding of the internal parts of the cerebrum, we must keep in view the relation of the nerves, and must class and distinguish the nerves, and follow them into its substance. If all ideas originate in the mind from external impulse, how can we better investigate the structure of the brain than by following the nerves, which are the means of communication betwixt the brain and the outward organs of the senses?

The nerves of sense, the olfactory, the optic, the auditory, and the gustatory nerve, are traced backwards into certain tubercles or convex bodies in the base of the brain. And I may say, that the nerves of sense either form tubercles before entering the brain, or they enter into those convexities in the base of the *cerebrum*. These convexities are the constituent parts of the cerebrum, and are in all animals necessary parts of the organs of sense: for as certainly as we discover an animal to have an external organ of sense, we find also a medullary tubercle; whilst the superiority of animals in intelligence is shewn by the greater magnitude of the hemispheres or upper part of the cerebrum.

The convex bodies which are seated in the lower part of the cerebrum, and into which the nerves of sense enter, have extensive connexion with the hemispheres on their upper part. From the medullary matter of the hemispheres, again, there pass down, converging to the crura, Striæ, which is the medullary matter taking upon it the character of a nerve; for from the Crura Cerebri, or its prolongation in the anterior fasciculi of the spinal marrow, go off the nerves of motion.

But with these nerves of motion which are passing outward there are nerves going inwards; nerves from the surfaces of the body; nerves of touch; and nerves of peculiar sensibility, having their seat in the body or viscera. It is not improbable that the tracts of cineritious matter which we observe in the course of the medullary matter of the brain, are the seat of such peculiar sensibilities; the organs of certain powers which seem resident in the body.

As we proceed further in the investigation of the function of the brain, the discussion becomes more hypothetical. But surely physiologists have been mistaken in supposing it necessary to prove sensibility in those parts of the brain which they are to suppose the seat of the intellectual operations. We are not to expect the same phenomena to result from the cutting or tearing of the brain as from the injury to the nerves. The function of the one is to transmit sensation; the other has a higher operation. The nature of the organs of sense is different; the sensibilities of the parts of the body are various. If the needle piercing the retina during the operation of couching gives no remarkable pain, except in touching the common coats of the eye, ought we to imagine that the seat of the higher operations of the mind should, when injured, exhibit the same effects with the irritation of a nerve? So far therfore from thinking the parts of the brain which are insensible, to be parts inferior (as every part has its use), I should even from this be led to imagine that they had a higher office. And if there be certain parts of the brain which are insensible, and other parts which being injured shake the animal with convulsions exhibiting phenomena similar to those of a wounded nerve, it seems to follow that the latter parts which are endowed with sensibility like the nerves are similar to them in function and use; while the parts of the brain which possess no such sensibility are different in function and organization from the nerves, and have a distinct and higher operation to perform.

If in examining the apparent structure of the brain, we find a part consisting of white medullary Striæ and fasciculated like a nerve, we should conclude that as the use of a nerve is to transmit sensation, not to perform any more peculiar function, such tracts of matter are media of communication, connecting the parts of the brain; rather than the brain itself performing the more peculiar functions. On the other hand, if masses are found in the brain unlike the matter of the nerve, and which yet occupy a place guarded as an organ of importance, we may presume that such parts have a use different from that of merely conveying sensation; we may rather look upon such parts as the seat of the higher powers.

Again, if those parts of the brain which are directly connected with the nerves, and which resemble them in structure, give pain when injured, and occasion convulsion to the animal as the nerves do when they are injured; and if on the contrary such parts as are more remote from the nerves, and of different structures produce no such effect when injured, we may conclude, that the office of the latter parts is more allied to the intellectual operations, less to mere sensation.

I have found at different times all the internal parts of the brain diseased without loss of sense; but I have never seen disease general on the surfaces of the hemispheres without derangement or oppression of the mind during the patient's life. In the case of derangement of mind, falling into lethargy and stupidity, I have constantly found the surface of the hemispheres dry and preternaturally firm, the membrane separating from it with unusual facility.

If I be correct in this view of the subject, then the experiments which have been made upon the brain tend to confirm the conclusions which I should be inclined to draw from strict anatomy; viz. that the cineritious and superficial parts of the brain are the seat of the intellectual functions. For it is found that the surface of the brain is totally, insensible, but that the deep and medullary part being wounded, the animal is convulsed and pained.

At first it is difficult to comprehend, how the part to which every sensation is referred, and by means of which we become acquainted with the various sensations, can itself be insensible; but the consideration of the wide difference of function betwixt a part destined to receive impressions, and a part which is the seat of intellect, reconciles us to the phenomenon. It would be rather strange to find, that there were no distinction exhibited in experiments on parts evidently so different in function as the organs of the senses, the nerves, and the brain. Whether there be a difference in the matter of the nervous system, or a distinction in organization, is of little importance to our enquiries, when it is proved that their essential properties are different, though their union and cooperation be necessary to the completion of their function—the development of their faculties by impulse from external matter.

All ideas originate in the brain: the operation producing them is the remote effect of an agitation or impression on the extremities of the nerves of sense; directly they are consequences of a change or operation in the proper organ of the sense which constitutes a part of the brain, and over these organs, once brought into action by external impulse, the mind has influence. It is provided, that the extremities of the nerves of the senses shall be susceptible each of certain qualities in matter; and betwixt the impression of the outward sense, as it may be called, and the exercise of the internal organ, there is established a connexion by which the ideas excited have a permanent correspondence with the qualities of bodies which surround us.

From the cineritious matter, which is chiefly external, and forming the surface of the cerebrum; and from the grand center of medullary matter of the cerebrum, what are called the *crura* descend. These are fasciculated processes of the cerebrum, from which go off the nerves of motion, the nerves governing the muscular frame. Through the nerves of sense, the *sensorium*, receives impression, but the will is expressed through the medium of the nerves of motion. The secret operations of the bodily frame, and the connexions which unite the parts of the body into system, are through the cerebellum and nerves proceeding from it.

SANFORD'S

Additional observations on the Peruvian Bark.

SIR,

MY son, called at your office in August last, in hopes of having a personal interview with you: intending at the same time, to present to you specimens of the different Barks and their preparations referred to in my printed Letter; but in consequence of your absence he had not the opportunity of making those explanations concerning them, which to him seemed necessary and therefore left the letter only. I have therefore thought proper to forward those specimens, together with some remarks, which seem necessary to the further illustration of the subject. Should the specimens of bark now presented, and the explanations given of them in my letter, serve to elucidate enquiries concerning this branch of the Materia Medica; my views in laying them before you will be fully answered. And should the remarks now submitted be thought by you to deserve a place in the Baltimore Medical and Philosophical Lycæum, your insertion of them in that useful work will prove to me satisfactory.

The box which covers this, contains the principal Barks which I have subjected to experiment and enquiry; and I have strong reasons for believing that this collection comprehends nearly every variety of Peruvian Bark of any importance, which is now to be found within the United States.

I regret that the samples of No. 1 and of No. 3, are so limited, the former could be procured only from a practitioner, residing in Virginia, and was forwarded in a letter. The latter has been handed out to friends, until the quantity left is very small. These specimens will however serve to shew the colour and configuration of each.

The powdered sample of No. 1, has been on hand and exposed to the air, for nearly ten years, and is not in its present condition thought to be very active. All the others are recent and deemed to be in a good state of preservation.

I am unable to furnish any specimens of No. 2, or of No. 7, which are referred to in my printed letter, there being none to be found in market.

The objects held up to view in my letter on the subject of the bark, are naturally arranged under the following heads, to wit:

First. To establish this fact, that several varieties of bark have lately been brought into use, differing materially from those kinds which had heretofore been known and approved; and that many of those possess but feeble medicinal powers, whilst others are totally destitute of any properties nearly resembling the true bark.

Second. To point out the characteristic appearances of each kind both in the crude and pulverized state in such manner as to render them most easily distinguishable by the senses.

Third. To shew that the middle coating or that part lying between the epidermis and semi-ligneous or cortical, where the glands reside, and the fluids are digested and elaborated, is that portion which contains the chief active principles of the bark; and to prove that this glandular or parenchymatous portion is capable by mechanical means, of being separated in a good degree from the remainder.

Fourth. To ascertain their comparative medicinal powers, as well by noting their effects on the diseased subject, as by the quantity of soluble principle which each affords to given menstrua.

Fifth. To ascertain by chemical tests, the analogy which subsists between the dry extract or essential salt of bark and the saturated infusion, from which it is obtained; and shew by deductions from actual practice the medicinal value of this preparation.

Sixth. To inculcate the propriety and importance of administering the bark in the atonic state of disease in proportion to the lost excitement; and shew that this object might generally be attained by adequate injections of this substance, even when the stomach would not retain it.

It seems now necessary to state only the result of subsequent observations made respecting the medical use of the essential salt of bark; to describe more particularly the manner of preparing this article; and to add some further particulars by which a Cincona may be most readily and accurately distinguished from every other vegetable substance.

The diseases which have occurred during the late summer and autumn, have furnished considerable opportunity for further use of the essential salt of bark, the effects observed in the treatment of intermitting fevers, correspond nearly with the opinion given of this preparation in my printed letter. It ought however to be remarked, that fevers of this description which were checked by this extract, were observed to recur more speedily after it was discontinued and generally in a greater proportion of cases than when the powder was liberally employed.

I readily concur in opinion with Vanquelin, that the effects produced by the essential salt are not proportional to the bark from which it is taken; neverthelesss I think that this preparation ought to be deemed of considerable importance. I have several times witnessed its great efficacy in the last stages of cholera infantum, as well as in other infantile complaints, of both acute and chronic forms. As it is soluble in nearly the same quantity of water as sugar, it is remarkably convenient for the use of children. It sits easier on the stomach, and is less liable to prove cathartic, than the powder. Owing to its small bulk and convenient form for exhibition in pills, it can be used in a disguised form, where the powder would be rejected.

I have known it produce very happy effects, given in this way, where the naming of bark occasioned the most distressing loathing. The extract when dissolved in much water, and kept in a warm situation is speedily decomposed and becomes mouldy; therefore aqueous solutions of this substance ought to be used (especially in summer) within two or three days after being made.

In the preparation of the concrete or essential salts of the bark great care and delicacy of management are required through every stage of the process.

The cold infusion ought to remain on the pulverized bark, until the liquor is very transparent, say from twenty to thirty hours; the sesond infusion from thirty to forty, and the third from forty to fifty hours. The clear liquor is then to be decanted, and after being filtered through several folds of fine flannel or bibulous paper, is to be evaporated in earthen plates, in a degree of heat which is not painful to the hand of the operator, until about one half the quantity be reduced. At this point of evaporation the liquor ought to be decanted with a steady hand into other plates to free it from an earthy looking sediment, which is thrown down by the heat. This part of the operation, must be repeated, say once or twice more, or as often as further sediment appear. When the liquor is reduced to the consistence of thick syrup, the plates containing it are to be removed from the bottom of the furnace, and placed above on slatted scaffolds, until the salt is dry and breaks up in brilliant scales.

It is only in a cool and dry atmosphere, that this substance can be removed from the plates in a dry state,by reason of its great eagerness to absorb moisture. Infusions of bark undergo the putrefactive fermentation during the summer months in so short a time, that it frequently will happen that the first infusion only can be improved. For this reason as well as on account of the deliquescent property of the salts, the temperate seasons are to be preferred for manufacturing this article.

I have hitherto confined my operations to the preparation of the dry extract by slow evaporation according to the plan of the Count De Garaye, its inventor; and which that author and others have denominated the essential salt of the bark.

Should opportunity permit, I propose hereafter to attempt the preparation of the salts in the white and chrystalized form, as described by Vanquelin, to be done by the Deschamps in France. I design likewise to prosecute enquiries already begun respecting the properties and use of the dry extract of bark, which is obtained by means of Alcohol. The result of which trials will in due time be submitted.

Until the experiments of Monsieur Vanquelin fell into my hands, I was persuaded that every bark, which, when treated with water, precipitated galls, and not animal glue, was changed by solutions of iron to a darkish green colour, and imparted little or no colour to cold water, was a Cincona, and that such vegetable substances as did not afford these precise appearances when treated in the same way, contained little or none of the kinic principle. I was led to this conclusion because it was supported by some respectable authorities, and because every genuine bark, of known and approved virtues, of whatever colour, which I had examined, afforded these phenomena in a conspicuous manner; and because other barks resembling the Peruvian in their sensible properties of bitterness and astringency (of which kind are the barks of the Cornus Florida, and Salix Alba) will not precipitate galls, and turn iron green, but will form a copious precipitate with gelatin.

The Angustura and the lately introduced bark under the name of Alcornoque, are the only two kinds which I have found when tested as above, to agree nearly with the Peruvian bark. These being of the growth of the same country with the Peruvian bark, and being equally tonic, and perhaps febrifuge too, it is not unlikely that by further experience they will be found to belong to the family of the Cincona. If the chemical experiments of Vauquelin are correctly stated, and those barks which he found to precipitate Isinglass and not Galls; and also to give out to cold water high colours, are barks containing the febrifuge principle as he intimates; it would seem that the tests here contended for were made doubtful; or that a highly predominating astringency was in these particular barks held in combination with the kinic principle. But as the author has furnished no satisfactory evidence, derived from actual observation of their medicinal effects on the human system; and as many other astringent barks which possess no febrifuge power, agree with them in chemical results; it may with propriety be doubted whether these supposed Peruvian barks, have a stronger claim to our confidence as febrifuge medicines, than the bark of the Oak, Persimmon, and numerous others. I think it may therefore be safely assumed as before stated, that every bark whose cold infusion is not higher coloured than a faint Orange, which forms a copious whitish precipitate, with the infusion of galls, little or none with solutions of Isinglass, and which is changed by a solution of red or green sulphate of iron, to a darkish green (not black or brown) is the true Cincona.

The best red bark marked No 1, and the first quality of yellow No. 5, produce when treated alike, results so similar to each other in regard to appearance of precipitate and colour of fluids, that they are not in this way easily distinguished.

The long time required for the powder of the red bark to subside from its containing water, probably has occasioned some mistake as to the colour of its cold infusion.

It is highly desirable that some means should be discovered of ascertaining the medicinal powers of different kinds of Peruvian bark, with greater certainty than can be done by the taste and other senses; and more readily than by observing the quantity of extract which they afford, or by the still more tedious means of observing their effects on the diseased body. I had hoped that those agents which serve to identify the principles sought for, would likewise shew the quantity of those principles. But I have not as yet been able to ascertain the quantity, by means of such of them as shew the existence of the principle. For infusions of bark, whether they be made strong or weak, afford with galls nearly the same quantity of precipitate; and weak infusions when united with iron, do not differ visibly in colour, or otherwise, from stronger ones treated in the same way. This branch of our enquiry in particular, requires further investigation; and I hope it will be prosecuted by those who have more leisure, than commonly falls to the lot of a country practitioner.

With sentiments of great esteem,
I remain,
Your obedient servant,
CLARK SANFORD,
Greenwich, *Connecticut*, *7th January*, 1812.

Doctor Nathaniel Potter.

…………

To the Editor of the Baltimore Medical and Philosophical Lycæum.

Washington, 3d March, 1812.

Sir—Having lately received by the public ship Constitution from France, various intelligence, particularly of a medical and scientific kind, I send you, for the information of the learned faculty of Maryland, an article contained in the *Journal de Medecine* for September last. It is presented to you in the form of translation and abstract; and is intended to introduce if possible, something preferable in the case stated, to the fashionable remedy quicksilver. With great esteem and regard yours.

SAMUEL L. MITCHILL.

Cure of Syphylis by Alkalies.

Professor Percy of Munich, has published a tract on the composition of the anti-syphylitic tincture of M. Besnard.—This B. is a knight of the civil order of merit, under the crown of Bavaria, chief of the general inspection of hospitals there, and member of the medical committee, &c. &c. &c. He has made known the secret of a thirty years practice in the treatment of lues. Its composition is the following:—

Take of purified salt of tartar, and
Of simple cinnamon water, each, one pound;
Of the purest opium, two ounces;
Of cinnamon water with wine, four ounces;
Let them be separately dissolved, and afterwards mixed

together, and exposed to a water bath, for three weeks, frequently stiring them.

Of choice gum arabic, two ounces;
Of volatile alkaline salt: one ounce;
Dissolve them in simple cinnamon water, six ounces;

Mix them all together, and let them stand in a well stopped vessel, for some days, at rest, and then be filtered and kept for use.

This medicine is employed for the removal of the venereal disease in all its forms and stages. Its dose is twenty-four drops, thrice in twenty-four hours, in a cup of the decoction of mallows roots, cool. The dose is diminished, when in the course of a few days the symptoms shall be mitigated; until at length, twice or even once a day, will be sufficient for its exhibition.

The quantity must be proportioned to the age and strength of young persons and children. The vehicle for such may be, the syrups of maidenhair, or the milk of almonds.

Where there are local affections, such as venereal ulcers, knobs, lumps, excrescences, phymosis, paraphymosis, and the like, the external application of the remedy, must be added to the internal. And for this purpose a decoction of marsh mallows roots with the same quantity of tincture as was described in the beginning, is to be employed in the form of a bath, embrocation, lotion, gargle, or topical application, according to the nature of the part affected.

Where there are mere ulcerations, they must be moistened, by means of a brush, or pencil, and if they do not heal under the action of the before-mentioned mixture, they must be touched daily with lapis infernalis, and dressed with lint.

If the sores treated in this manner, although clean and not spreading, heal and consolidate but slowly, recourse should be had to restorative diet and strengthening medicines.

Buboes are to be moistened twice a day, with the same tincture, and after some days are employed after this manner, they are to be covered with a plaster made of one ounce of diachylon cum gum. and half a dram of castile soap, until the swelling disappear, or suppuration ensue.

The same plaster is good for the swelling of the testicles arising from repressed gonorrhæa. Yet, in cases of this kind the suspensors must not be neglected.

In the inflammatory stage of gonorrhæa, the affected part should be bathed as before directed; and four pounds of the mallows decoction, and twenty four drops of the tincture be taken daily into the stomach. The inflammation being thus subdued, the tincture is to be taken three times a day as first directed in the mallows decoction; and at the same time, injections made of a dram of pure alum, four ounces of the mucilage of gum arabic, and a pound of distiled water, may be employed, to the urethra in men and the vagina in women, three or four times a day, and if in the progress of the cure the discharge does not cease, a scruple of the extract of lead, may be added to the remedy.

The recovery of the patient will always be expedited, by bathing the whole body in warm water, impregnated with a measure of common lee, or a portion of potash, as well as by a proper regard to food and drink.

As to other symptoms, when they occur, the physician must treat them according to his judgment, and the rules of art.

Such is the practice in this formidable malady, which is reported to have extended over a great part of Germany, the departments of the Rhine, the military hospitals of Bavaria. and the kingdom of the same name.

M. Besnard is reported to be the archiator, or first physician to Maximilian Joseph, King of Bavaria. By publishing this remedy, he is extolled as having bequeathed a rich legacy to suffering humanity. His sovereign has issued a certificate or proclamation in these words. "Maximilian Joseph; by the grace of God, king of Bavaria, &c.; after the great number of successful trials made in different places, and by different physicians and surgeons, with the anti-syphylific tincture of M. de Besnard, in all sorts of venereal diseases, whereon is founded the definitive conclusion mentioned in the report of the administrative direction of Bavaria; We testify by these presents, to its inventor M. de Besnard, chief of the general inspection of all the military hospitals, our high approbation of the important service he has rendered to humanity, and we further assure him of our entire good will, and of our sovereign protection."

"*Munich*, 8*th May*, 1808." (Signed.)

It will immediately appear to the experienced practitioner, that the important and essential ingredients of this composition are the POTASH and OPIUM. The cinnamon water, the gum arabic and even the ammoniac appear to be of little value in the receipt, and as it is roundly affirmed that M. de B. has long since discarded the use of mercury in the cure of syphylis, let the form of it be as it might and the mask ever so imposing and deceptive, we may conclude that the *fixed alkaline salt* and the *juice of the poppy*, are capable of eradicating this formidable disease.

It may be also noticed, that if this anti-syphylitic preparation should be considered as novel, the source whence he derived it will not be deemed so. It, will immediately occur to the well read physician, that *Professor Peyrilhe*, of Paris, attempted as long ago as 1769, to substitute the alkaline for the mercurial practice; and that in 1780, he published a book on the advantages of that method of treatment. The curious and pungent matter which it contained, raised against him a host of critics, detractors, and enemies. *Peyrilhe*, it is said, prescribed indiscriminately, the concrete and liquid ammoniac. This was the ground work of his practice, and what he added to the alkali, is merely incidental or collateral.

Hence it must be admitted that there is a close analogy between the doctrine and practice of Messrs. *Peyrilhe* and *Besnard*. Still it does not follow that the latter had copied the former. There is nevertheless, one very remarkable difference between them. *Peyrilhe*, exhausted perhaps by the attacks which the publication of his book had drawn upon him, or possibly undeceived as to the properties he had ascribed to his remedy, after some years gave up all further trials and resistance: while *Besnard*, more and more convinced of the efficacy of his prescription, to which he added opium, at the time when it was alledged that that substance would of itself work a cure, persevered in his opinion, and made all possible exertions to propagate it. He does not ask any person to credit him, on his mere declaration. He boldly appeals to experience and to facts; and solicits the judgment of the wise and the enlightened on the case. He has not protected himself by any patent or monopoly; but has published his mode of cure for the benefit of mankind, like a citizen of the world.

In justice to our own country, I must mention that the alkaline practice has been successfully pursued since the year 1798, in New York, for the cure of venereal ulcers, and other symptoms. The memoirs and statements contained in the first six volumes of the Medical Repository, will satisfy those who wish information on this subject, with what neatness and benefit, they have been employed in the great Hospital there.

....

Williamson on Scrofula—in continuation.

AFTER a consumption has formed, or even when the lungs are much predisposed, if it be of an inflammatory nature, recourse should be had to depleting remedies and to an abstemious diet; the former reduce the action of the system by depriving it of a part of its excitement, and the latter decreasing the quantity of chyle, necessarily furnishes less excitement. By these means inflammatory tumors sometimes terminate by resolution. If a suppuration should supervene, we ought to be careful not to pursue depletion too far, for no abscess can form healthy pus, if the tone of the interested solids are too much reduced, nor will an abscess heal if it do not form healthy pus. This is an obvious fact in external abscesses, and it can be no less true in those of the lungs and other internal parts of the system. If the consumption be of a scrofulous nature, a contrary mode of treatment is requisite.

The scrofula is a disease of debility, and any thing that debilitates the general system, or parts affected, increases the disease. When scrofulous tumours terminate in suppuration, the pus is generally bad, and is not to be improved but by increasing the tone of the solids; this is to be done by proper diet and strengthening medicines. In external scrofulous tumors which have suppurated, the bark has been proved to be very salutary; that it should be less so in those of the interior of the system, can only be accounted for from the peculiar situation of the disease, and not from any organ or viscus prohibiting its use. A healthy suppuration is as requisite in a scrofulous tumor as in a phlegmon, and it is as impossible for it to heal without it. In both cases it is desirable to prevent a suppuration, but if this cannot be done we are next to endeavor to produce a healthy one.

Exercise is very conducive to health, which cannot be supported without it. In diseases of the lungs when exercise becomes expedient it should be gentle. Sydenham speaks in the most exalted terms of riding on horseback in consumption. Coughing is always to be dreaded when severe, or of long continuance, whether there be an actual disease of the lungs, or merely a predisposition to one. If the person thus affected be of a scrofulous habit, he cannot be too solicitous about his fate. In cases of this nature, every effort should be used to prevent coughing. I have tried almost every remedy that has been hitherto recommended to allay and prevent coughing in patients of a scrofulous habit, who have been laboring under this affection, but have found none so successful as a composition of gum opii. G. assafœtida et Tart. Ant. This composition has an admirable effect, especially if the nervous system participate in the disease.

Emetics have lately been recommended in consumption. The public fame of some writers is so great that their wildest theories are received as sacred by the great herd of readers, and the most implicit confidence placed in every thing they say. although their doctrines be indelibly stamped with error. I should have supposed that intuitive conviction against this practice would have been sufficient to have arrested it in its forming state, had man been deprived of the faculty of reasoning. That neither intuition nor reason have been sufficient to withstand an error of so much magnitude is to me incomprehensible. After taking a view of the structure of the lungs, the circulation of the blood, and the effect of an emetic on the lungs, we would suppose him an ignorant physician indeed who would prescribe an emetic in the consumption or hemoptysis. From the information we now possess we can not draw this inference, although we are fully persuaded that it is a bad practice, for we find men of reading and knowledge, and who have an extensive practice, complying with this theory: But

"Knowledge and wisdom, far from being one,
Have oft-times no connexion. Knowledge dwells
In heads replete with thoughts of other men;
Wisdom in minds attentive to their own."

In my history of the Influenza of 1807.* I have been led into a view of the use of emetics in that disease; the observations there made are equally applicable here. When writing that history I expected to have taken a more extensive view of the use of emetics in the disease now in question than my present engagements will admit of. I shall, however, make some additional remarks.

---

* Medical Repository—3d Hexade, 1st vol

Important cases might be given from ancient and modern authors in confirmation of the injurious effects of emetics in consumption. Tulpius mentions a physician whose lungs had long been diseased, who expired soon after taking an antimonial; he attributes the death of the patient to the effect of the emetic. I have myself known several cases of hemoptœ where much injury has been done by emetics; and I felicitate myself on never having been accessary to such a practice.

June, 1809—At this period I have under my care a patient who confirms the truth of the above sentiments. This patient is a delicate female who has for several years been predisposed to pulmonary affections, and who has more than once laboured under a severe disease of the lungs. A few weeks since she contracted a violent catarrh, attended with a troublesome cough, pain in the breast, spitting of blood, &c. She was bled, had a blister applied to the breast, which was kept open for some time; antimonials, demulcents, diluents, &c. were exhibited. In a few days the spitting of blood ceased and the other pneumonic symptoms began to give way. About this period the warm weather commenced, and she was attacked with a bilious fever. The puking was considerable and attended with much exertion. The consequence was a return of the hemoptœ, with the other pulmonary symytoms which continued for several weeks. When the bilious attack took place we had to alter our mode of treatment and prescribe for this disease, which was soon subdued. Afterwards we recurred to our former remedies which had finally the desired effect.

I am not disposed to make these strictures on the use of emetics in diseases of the lungs in general; although I believe there are but few cases where they can be administered with advantage, if with safety; but I can not conceive them safe where there has been hemoptysis or where we have any reason to apprehend a disease of this nature.

Diet.—Much might be said under this head, but I shall confine my remarks to the article of milk.

Milk is very easy of digestion and at the same time very nourishing, and must therefore be salutary in consumption. That of a healthy woman is preferable when attainable. I knew a lady who was so much reduced by a fever that her life was despaired of by her physicians; she craved the milk of a woman, it was procured her, and she recovered. Van Swieten says "a young lady of high quality has used human milk for a year and more, with so good effect, that the cough, the purulent spitting, weakness, and emaciation, have quite ceased and she is alive in health and vigour." That milk will support adults for a long time, even when no other nourishment is taken has been proved. "A Roman lady of birth was accused of a capital crime before the Pretor. She was condemned to death and delivered to the triumvir, who ordered her to be carried to prison, and there to be strangled according to custom. The tears of the criminal influenced so powerfully the jailor, charged with the execution, that he resolved to let her perish with want and famine in the prison, rather than take away her life. He did more—he permitted her daughter to visit her, but took care that she did not take her any thing to eat. Some time elapsed and the jailor was surprised that his prisoner lived so long without taking nourishment. He observed her daughter with more attention. What a sight, he saw this generous daughter suckling her unfortunate mother to prolong the dreadful death to which she had been decreed by her humane jailor.

This heavenly act of filial love was rewarded by the magnanimous Romans in the most honorable manner. The mother and daughter were ordered to be supported the remainder of their days at the public expense, and a temple was directed to be built near the prison, dedicated to piety!

All the distinguished ancients eulogized milk as one of the best dietetic articles. Aretœus says, "milk is pleasant to the taste, easy to be drank, affording a substantial nourishment, and more familiar from child-hood, to the body than any other food: besides its colour is agreeable to the sight; it causes no irritation to the organs of respiration; it lubricates the throat, and makes the expectoration of phlegm easy; it keeps the bowels lax; it is a pleasant remedy for ulcers, and more mild and salutary than any other. If any one drink a great deal of milk he will need no other food." Many of our most distinguished modern authors have also past the highest eulogies on milk. Among these I know of none who has treated this subject more interestingly than Tourtelle.

As this is a very interesting subject, and one with which the public are not sufficiently acquainted, permit me to give you my translation of Tourtelle on Milk.

"Milk is the first aliment of man and of the greater part of animals; it is the most proper food in the first age, and especially that of the mother. It is an animal emulsion, a white liquor, of a sweet taste, which is formed of three distinct parts, viz. of the serum or whey, of the cheese and butter. The serum is the vehicle of the other two substances, it contains a sweet substance called the sugar of milk, which is, as it were, a sugar in its forming state. It also contains a calcarious phosphat which is more abundant in it, under similar circumstances, than in the other animal humors. It is the phosphat of lime that forms the case of the bones; this is the reason nature has placed it so abundantly in milk, for the growth and development of the bones, in the early periods of life. Cheese is a real albumen, and butter a concrete fixed oil; the concretion of which, and the separation from the other parts of the milk, by simple motion, are owing to the absorption and to the fixation of the oxygen of the atmosphere, during the formation of cream.

The proportions of serum cheese and butter are nearly the same, in the milk of the cow and that of the goat; that of the latter however contains a little more cheese than the former; and that of the former a little more butter than the latter. The milk of the goat has a twelvth or thirteenth more of the sugar of milk than that of the cow. Their properties are nearly the same; however, that of the goat is not so relaxing and is a greater tonic, it sometimes passes off slowly and even constipates the bowels. The milk of sheep contains a fourth more of cheese, twice as much butter, and a third less sugar; its cheese is more tenacious, and consequently the milk not so easy to digest. That of the ass contains a third more of the sugar of milk than that of the cow, and but very little cream from which butter can be made. Spielman was able to obtain from a pound of asses milk, but a dram and a half of cheese ; it was very nice. These three substances separate spontaneously by standing, but no coagulum can separate this milk, which is not so fat and rich, in cheese, as that of women. Spielman obtained from two pounds of woman's milk an ounce and an half of cream, which gave six drams of butter and half an ounce of very agreeable cheese. Mare's milk has the greatest affinity with asses; but what is very important to remark, is, that the proportion as well as the medical properties of milk, are owing to the actual state of the animal, to the nature of the food with which it is nourished, and to the passions it experiences.

Milk is an aliment adapted to the digestive forces of the new born child; it is the most proper for it, in consequence of the facility with which it is digested. That of the mother is the most proper, and no other answers fully as a substitute: In fact, the best nourishment for a child is that with which it commences its existence, and to which nature has gradually given the most proper preparation for its state, and for the development of its organs. Suckling has its limits, and it is as improper to suckle a child too short a time as too long. In general, suckling should not be discontinued in less than seven months, nor continued longer than twelve or fifteen, and the custom of not giving the breast more than nine months, is formed from experienced observation. Suckling continued beyond a year or fifteen months, promotes or augments a disposition to the rickets, especially when dentition is performed slowly and with difficulty. It is not proper to give to children during the three or four first months any other nourisment than milk. It is not until the end of this period that other diet should be added, and when it is the quantity should be gradually augmented until the period of weaning. By adopting this plan we are not obliged to make great changes,and it is a real advantage, seeing that great changes generally occasion obvious, and sometimes fatal derangements in the system. There are signs by which we judge of the good and bad qualities of milk. Galen judged it by the taste, smell and appearance: he supposed that to be good which was of a sweet taste and without odour, or of an agreeable smell: he required it to be white, regular, and of a tolerable consistence. When we examine a small drop of it, it ought to preserve its round form; he considered that pernicious which was too thick or too thin, irregular, and especially that of which the taste was either bitterish or saltish. Primrose tried it by soaking a white linen in it, which he afterwards dried; and proscribed, with reason, that which communicated to the linen any colour whatever, except that which is natural to the milk.

When milk does not possess the natural qualities, it digests badly, and occasions acidity, colic, diarrhæa, convulsions, mesenteric obstructions, hectic, &c. When the bad qualities of the milk cannot be corrected by regimen, it is necessary to choose another nurse. Acidity and the other affections of which I have first spoken, are also frequently owing to the excessive use of vegetables, and soups; and at other times to the extreme weakness of the digestive organs. In the first case the nourishment of the child should be changed; in the second, which is generally known by finding the milk coagulated in the stools; the child should have some general tonic, and especially the watery infusion of rhubarb. Absorbent earths have been recommended by physicians, as an efficacious remedy in the acidities to which children are subject; but experience and observation have not justified this practice. On the contrary they have taught us that the use of these substances generally increase the disease, and give rise to more serious accidents.

The milk of ruminating animals, which contains the most cheese, may, to a certain degree, supply the use of that of the mother, but it is never so salutary. It is only in cases where the child cannot be nourished without being exposed to danger, or where the milk is not proper for it, that the child should be weaned. In these instances, I prefer the milk of quadrupeds to that of a mercenary nurse; there are too many risks to be hazarded with the most of these women, who are compelled by a sordid interest or want, to renounce the quality of a mother, without speaking of the pernicious passions to which many of them give up, of their bad conduct, a want of care, and of many other similar causes which have a great influence on the children whom they nurse. How many women are there who transmit with their milk, the diseases and vices with which they are infested, and who entail on children a painful and miserable existence if death does not relieve them in early infancy?

Milk is a salutary aliment prepared by the hands of nature, and as it were, half digested. It is salutary, especially to persons whose digestive organs are very weak, as well as in all cases of exhaustion, in most cases of phthysic, cough, and of marasmus. A disposition to these diseases, and even the progress of time, have sometimes been arrested by a milk diet and proper exercise without any other remedies. The exercise should be proportioned to the strength of the patient. There are however cases of the same diseases where this aliment is not proper, as has been already well observed by Hippocrates: "Lac dare capite dolentibus, malum; malum vero etiam febricitantibus et quibus hypochondria elevata sunt murmurantia, et siticulosis; malum autem et quibus dejectiones biliosæ et quæ in acutis sunt febribus, et quibus copiosi sanguinis facta est ejectio. Convenit vero tabidis non admodum valde febricitantibus lac dare, et in febribus longis et languidis, nullo ex supra dictis signis præsente, et præter rationem quidem extenuatis. (Aph. 64, sec. v.)*

Milk is improper in most of the cases mentioned in this aphorism, because they are accompanied with, or produced by the presence of gastric or intestinal saburra; consequently these substances, by altering the milk, augments the focus of irritation and corruption, and the disease is aggravated. For this reason Hippocrates also says: impura corpora, quo magis nutriveris, eo magis lœdes. (Aph. 10, sec. ii.)†

Milk is not proper, according to Hippocrates, to very thin persons, because the digestive juices have not a sufficient energy, and the powers of the stomach are insufficient to digest it; hence it acquires pernicious qualities, and gives place to nidorous, or acid saburra, to the colic and to a diarrhæa, which shortens their lives. A high fever also renders the use of milk improper, because the labour of digestion cannot then be properly performed, hence the milk is badly digested and becomes a new morbid stimulus.

The best manner of taking milk in the cases of which I have just spoken, is to suck the animal, or to swallow the milk just as it comes from the udder, or to warm it to the degree of heat which it has when first milked. But the two first are preferable, because the milk then still retains its flavour, and is, in this state, much the most restorative. It should never be boiled, for it then becomes much more difficult to digest, and loses its flavour.

---

* "To give milk to those affected with head-ache, is improper; nor is it adapted to the feverish, flatulent and thirsty; nor does it suit the bilious diarrhæa which occurs in acute fevers, where a great quantity of blood is evacuated. But it is proper for the consumptive, who have little fever, and it is salutary in slow, low fevers, continued beyond their ordinary period, when not contra-indicated by any of the preceding symptoms."

† The more food that is given when the humours are unconcocted, the greater is the injury committed."

The nourisment and constitution of the animal which furnishes the milk, are not objects of indifference: they have the greatest influence on its qualities, and merit the first attention. The milk of cows when in heat coagulates very difficultly. There are also many vegetables which eaten to a certain quantity by these animals, change the nature of the milk, and of its constituent parts. The shells of peas produce this effect. Milk preserves the colour, odour, taste, and most of the other properties of the aliments with which the animal is nourished. Saffron gives it a yellow colour, and madder a red: Thyme communicates its odour, and garlic its taste to it: Absynthium renders it bitter, &c. It were desirable then that the animal should only use plants which are proper in the species of disease for the cure of which milk is employed. There is no doubt but that by attending to these principles, we should succeed more certainly in curing and preventing diseases to which we ordinarily oppose but feeble arms, because these precautions are not taken in the use of this article of diet.

As to the constitution of the animal, it should be strong and robust; and should be of a suitable age, and in good health.

The milk of young animals is too thin, that of old ones too thick. The milk of an animal in heat is not good, nor that of an animal near the term of delivery, nor which has been lately delivered. The milk is better in the month of *Florial* and during summer than in winter: It is too thick in this last season.

Although milk is in general a very salutary aliment, its use alone, would not be sufficiently nourishing to strong and robust men, and to those who are habitually engaged in laborious work, it would not sufficiently support their energies, and would soon throw them into a state of debility and pernicious exhaustion. There are also, persons who do not digest milk, or who digest it badly, and to whom it is consequently injurious; in the first case, it produces constipation, and in the second, a sense of heaviness in the stomach, colic, and diarrhæa. To remedy the constipation, it has been recommended, to combine with the use of milk, that of mild laxatives or injections, and when it causes pain and diarrha, to mix it with water, and to take at the same time, some gentle tonic. I have seen, in this case, good effects from rhubarb in powder, which I prescribed twice a day previous to taking the milk. When these means are ineffectual, and the milk continues not to digest easily, it is absolutely necessary to give up the use of it.

Butter possesses the same qualities that the other fixed oils do: it is nourishing, but like these, it is not easily digested by weak stomachs. The habitual use of butter relaxes the primæ viæ, throws them into atony, and promotes abdominal herniæ. Butter, as well as the fixed oils, turns rancid by age, and becomes bitter. In this state, it should be rejected from use. Cream is only butter mixed with a certain quantity of cheese: it is more easily digested than butter, but it sours in stomachs that abound with acids, and it may then produce very serious effects.

The *caseous* substance, or cheese, is the principle nutritive part of milk; it is very analagous to gluten, of which it possesses nearly all the properties. Cheese made of milk deprived of its cream, is very nourishing, but not very soluble in the gastric juice. It is only proper for robust persons. It is a strong and healing aliment, but nourishing. Caseus robustus est et œstuosus et alit. (Hipp. de diæta, lib. 2nd.) That which is made of the entire milk is not less nourishing, and it is more easily digested; it is still more so when the cream is added, for the particles of the latter interposed between those of the cheese, diminish its coherence. That made of cream alone is not so nourishing, but digests easily.

Cheese is used, not only when it is fresh, but also when it has gone through some degree of putrefactive fermentation. In this last state it is more or less acid and stimulant, according to the degree of putrefaction which it has experienced. When this is advanced it promotes the development of the germs which certain insects have deposited in it. Some persons roast the cheese previous to eating it, but this process renders it more difficult to digest. In general, cheese should only be used in small quantities.

> Caseus ille bonus, quem dat avara manus.

In this manner it aids digestion by its putrefactive quality, or, rather by its promoting the solution of the aliments in the gastric juice. Cheese that is too old communicates in the primæ viæ, the leven of putridy. That which is too much pressed converts the aliment into acidity. Those made of sheep's and goat's milk digest more easily than that of cows. It is with the milk of sheep that *Ionches*' white cheese is made, and the cheese of *Raquefort*. That made of milk which has not been boiled is the best. Lastly, the cheese of fresh cream is cooling and digests very easily.

Serum or whey is the least nourishing part of milk; besides its sugar, which appears to be the basis of the *lactic*, *sacho-lactic*, and *acetous acids*, which modern chemists have obtained from whey, and some salts which it also holds in solution; It also contains a portion of butter and cheese, that may be separated by clarifying it with the white of an egg or fish glue. It is only used as a medicine, and not as an aliment.

The Tartars and Kalmuchs, from time immemorial have used a wine, and a species of very strong brandy, which they obtain from the milk of the mare, by means of simple agitation. The agitation is sufficient to make the milk pass through the vinous fermentation. Nicholas Oseretskowsky, of St. Petersburg, has made many experiments on this subject, of which the principal results are: 1st. Skimmed milk does not undergo the vinous fermentation, neither alone nor with a ferment. 2nd. Milk agitated in a close vessel contracts this fermentation, and is converted into wine. 3d. Lastly, Milk fermented by the effect of heat, loses its alcohol, and is changed into vinegar."

Extract from a report made to the class of phisico-mathematical sciences, on the 9th of Sept. 1811, by a committee, composed of Messrs. Hallé, Humboldt and Percy, on a memorial of Mr. Legallois, M. D. concerning the principle of action of the heart, and the seat of that principle. Translated from the Paris Moniteur by J. D. Synott, M. M. & C. F. M. and Professor of Belles Lettres in St. Mary's college.

M. LEGALLOIS has undertaken to prove, in the memoir which he presented to the class, that the principle of the action of the heart resides in the spinal marrow.

The committee, in the first place, give a succinct account of the divers systems suggested since the discovery of the circulation to explain the action of the heart. The several systems of Descartes, Sylvius, Delebre and Borelli, and that of Willis, as revised and corrected by Boerrhaave, the systems of Stahl, Ens and Stachelin, are noticed by them in their turn. But the one which Haller founded on irritability has particularly attracted their attention. As this last has consigned all the others to oblivion, it has given rise to lively discussions which have been continued to the present day. These disputes proceed from this fact; that Haller's system, as well as those that preceded it, are not sufficiently satisfactory to account for all the phenomena.

In taking a general view (say the committee) of the different systems invented before Haller, we may remark, that the nervous power, in one way or other, has always been considered as one of the essential conditions to produce the motions of the heart, and the seat of this power has constantly and uniformly been placed in the brain.

The cardiac nerves had of course a determinate use in all these systems, and it was easy to conceive how the heart was subjected to the influence of the passions; but they could not explain why the circulation continues in acephalous animals, nor why, in experimenting on animals, the interruption of all communication between the brain and the heart, does not stop the action of the latter organ. Since the days of

Haller, irritability has been the basis of every system: in considering this property as essential to the muscular fibre, and independent of the nervous power, the circulation in acephalous animals and the different phenomena observed in the experiments above alluded to, present no difficulty; but the use of the nerves of the heart, and the influence of the passions on that organ became inexplicable. The necessity of obviating these difficulties has split the advocates of irritability into two sects. On one side the zealous defenders of pure irritability have called to their assistance the most inconsistent hypotheses, and all their exertions tend only to prove how very difficult it is to defend the cause they have embraced. Their opponents have introduced the nervous power with irritability, and consider the latter as one of the functions of the former; but then they were necessitated to admit, either with regard to the seat, or the mode of existence of this nervous power, certain conditions, which as they themselves confess, are far from being demonstrated, on which they do not agree among themselves, and which, when applied to the action of the heart, do not entirely remove the old difficulties, or else give rise to new ones.

It is easy to perceive whence it is, that so little progress has been made in this long and important controversy. If we examine all that has been said on the subject since the time of Haller, we shall find nearly the same facts, the same experiments, and the same reasonings advanced by either party. The only new experiments are the application of Galvanism to stimulate the cardiac nerves, and even these are new only in appearance, as in the days of Haller electricity was employed for the same purpose and with the same results. It is evident that we have nothing more to hope for the advancement of this interesting part of physiology by following the track already beaten by so many celebrated men, for nearly sixty years. New paths must then be opened, and new methods discovered for the investigation of nature: above all, that degree of precision and strict logical reasoning must be introduced into physiological researches, which has been already employed with such success in the other departments of natural philosophy. This is what M. Legallois has endeavoured to effect.

It was not, at first, the object of M. Legallois to search into the causes of the movements of the heart. He was attached to the theory of Haller, until experiments, undertaken with quite a different view, led him to conclude that it was still a mystery to him how the nervous power, if it interfered at all, was interested in aiding the functions of the heart.

That we may give a clear view of his investigations, we will relate on what occasion, and by what chain of facts and reasoning he was induced to engage in this research.

A particular case of midwifery gave him, some years ago, a wish to know how long a full grown fœtus may live without breathing, counting from the moment when, from any cause whatever, it has ceased to have any communication with the mother. This question, so very curious in itself, and so interesting to the practice of midwifery and medicine, had scarcely been glanced at by authors. M. Legallois undertook to solve it by direct experiments. And in order that the solution might extend to as great a num ber of cases as possible, he placed the fœtus of different animals in such different situations as might resemble the principle accidents that may happen to the human fœtus whilst its communication with the mother is intercepted. Among these accidents there is one which but too often takes place; this is separating the head from the body in the delivery of a *footling* case. The author wished to know what became of the fœtus in this case, whether it died at the moment of decapitation, and of what death it died? He discovered that the trunk remains living, and that, by preventing hemorrhage by passing ligatures around the vessels of the neck, it does not die except in the same space of time, and with the same phenommena, as if, without being separated from the head, respiration had been completely intercepted. And what evidently proved to him that the decollated animal is only in a state of asphyxy, is, that he could at will prolong its existence by supplying respiration by means of the inflation of the lungs.

M. Legallois concludes from these facts, that decapitation does no more than arrest the inspiratory motions, and that consequently the principle of all these motions is in the brain, but that the principle of the life of the trunk resides in the trunk itself. Searching afterwards for the immediate seat of each of these principles, he found that the principle of the inspiratory motions resides in that part of the *medulla ablongata*, that gives origin to the nerves of the eighth pain, and that that of the life of the trunk has its source in the *medula spinalis.* It is not by the whole of the spinal marrow that each part of the body is animated, but only by that portion from which it receives its nerves: so that by destroying a portion of the spinal marrow, we deprive of life those parts only of the body that correspond with this portion. Besides, if we intercept the circulation of the blood in a portion of the spinal marrow, life languishes and soon becomes entirely extinct in all the parts that receive their nerves from that portion of marrow. There are then two means of destroying life in this or that part of an animal body; one by destroying the marrow from which the part receives its nerves, the other, by intercepting the circulation of the blood therein.

The result of the above was, that the maintenance of life in any part of the body essentially depended on two conditions; namely, the integrity of the corresponding portion of the spinal marrow, and the circulation of the blood: and consequently, that it was possible to continue life in any part of an animal as long as these two conditions could be made to subsist; that one might, for example, continue life to the anterior parts alone, after having deprived the hinder parts of life, by the destruction of the spinal marrow that corresponds with them, or to the hinder parts, after having, in like manner, killed the anterior parts.

M. Legallois wished to know if his experiments would confirm these consequences, and if it were in reality possible to enable this or that portion of an animal to live, independently of the other parts.

He first submitted to experiment a rabbit twenty days old; in this animal he destroyed all the lumbar portion of the spinal marrow. This operation not immediately interfering with the rest of the marrow, and the circulation, according to Haller, not being necessarily affected by it, there was sufficient room to expect, in reasoning from preceding experiments, that the animal would survive a considerable length of time; but in less than two minutes respiration was suspended, and in less than four minutes it no longer gave signs of life. The same experiment frequently repeated was always attended with the same results, without the possibility of preventing them by any means whatsoever, and it was invariably true that a rabbit twenty days old could not survive the destruction of its lumbar marrow. This was the more surprizing as rabbits of that age can continue to live after decapitation, that is, after losing the whole of the brain. This is the fact that M. Legallois could not reconcile with his preceding experiments, and which has led him to discover that the principle of the powers of the heart resides in the spinal marrow.

He first of all assured himself that the destruction of either the dorsal or cervical portion of the marrow was mortal to rabbits of twenty days old, as also of the lumbar portion, and even in less than two minutes. He next discovered that the same experiments repeated on rabbits of different ages did not produce the same effects. In general, the destruction of the lumbar marrow is not suddenly mortal to these animals before the age of ten days; many even of fifteen days survived it. With rabbits above twenty days the effect is the same as with those of twenty days. Very young rabbits may continue to live after the destruction of either the dorsal or the cervical marrow, but a much shorter time, and in fewer cases when the latter is destroyed, than the former. Those that have passed the age of fifteen days can survive the destruction of either the one or the other.

In all these partial injuries, even when death speedily ensues, it is never instantaneous, except in such parts as receive their nerves from the portion of marrow destroyed. It does not take place in the rest of the body until after a certain time; but this time is determinate, and no means can prolong it. This time, which is the same for animals of the same age and species, is longer in proportion as the animals are younger. For example when the cervical marrow is destroyed in rabbits, life is extinguished on the instant through all the neck, but it continues in the head, which we may ascertain from the spasmodic motions of the mouth. Life remains also in the posterior parts, from the shoulders downwards, as may be perceived by the sensations and voluntary motions they exhibit. On the day of birth, the motions of the mouth continue about twenty minutes, those of the rest of the body fifteen minutes; at the age of fifteen days, the spasmodic gaspings do not last longer than three minutes, nor are the motions of the body perceptible longer than two minutes and an half. In fine, at the age of thirty days all motions cease within a minute or a minute and an half. After the destruction of the dorsal marrow, it is the breast and not the neck that is struck with death; in the rest of the body, the same phenomena and duration may be observed. If the three portions of the marrow be simultaneously destroyed, the gapings, (the only signs of life that then are manifest,) have, in different ages, the duration above mentioned.

The author, who had so often practised decapitation on rabbits of different ages, had constantly remarked, that the head, when separated from the body, continued to gape, and that during a time proportioned to the age of each animal. Now this space of time was the same as after the destruction of the spinal marrow. But it is evident that after decapitation there is no longer any circulation in the head, and that the spasmodic gapings which take place in this case, do not continue longer than life subsists in the brain after the circulation has totally ceased.

This was what first indicated to M. Legallois, that when the partial destruction of the spinal marrow extinguished life in the rest of the body, it was by suddenly arresting the circulation. To make himself certain of this, he cut out, at the base of the large vessels, the heart of rabbits from the age of five or six days to a month; and having attentively remarked the duration of the different signs of life, counting from the moment when the circulation was arrested by the above method, he found that the spaces of time were exactly the same with those he had observed after the destruction of the spinal marrow. He might have considered this correspondence of time as sufficient to decide the question. But he wished to determine to a certainty, whether the circulation is really intercepted at the very moment when the spinal marrow is destroyed. The absence of hemorrhage, and the vacuity of the arteries were the most evident signs he could have of the fact, and he found that in effect, the carotids are empty immediately after this operation, and no blood was yielded on the amputation of the members, even when it was performed close to the body, and before life is extinct in parts the marrow of which has not been destroyed. In a word, all the signs that serve to give a knowledge of the state of the circulation proved to him that whenever the destruction of any portion of the spinal marrow suddenly causes death in the rest of the body, it is by arresting that function. This last effect takes place, not because the action of the heart ceases on a sudden, but because it loses all its force, so as not to be able to impel the blood into the carotids.

The result of all this is, that it is from the spinal marrow that the heart derives the principle of its powers, and from the whole of this marrow, since the destruction of any one of the three portions can, according to the age arrest the circulation, and the destroying of the three portions arrests it in all ages. Another result is, that each portion of this marrow has on life two modes of action very distinct from each other. One, by which it essentially constitutes life in all the parts that receive their nerves from it; the other, by which it serves to maintain life in all the parts of the body, by contributing to furnish all the organs that receive twigs from the great sympathetic, and especially the heart, with the principle of life and power, necessary for their functions.

It is plain then, that in order to enable the fore or hinder parts of an animal to live singly, (after having caused death in the other parts of the body by destroying the corresponding marrow,) it is necessary to be able to prevent this destruction from arresting the circulation. To effect this it is sufficient to diminish by ligatures passed round the arteries, the extent of the parts to which the heart has to distribute blood. We have seen, for instance, that the destruction of the lumbar marrrow is immediately mortal for rabbits that have attained or passed the age of twenty days; but they would not die of this operation, if, before we destroy the marrow, we were to commence by tying the abdominal aorta between the celiac and anterior mesenteric arteries.

The application of this principle to other parts of the body leads to a circumstance very singular in appearance; it is, that to be able to support life in rabbits of a certain age, after having destroyed the cervical marrow, we must begin by cutting off the head. They die inevitably, if without beheading them, we destroy this marrow. This fact ceases to surprise, when we remark that by decapitation we remove the entire head from the dominion of the circulation, and that by this means the heart having need of less force to continue its functions, we may weaken it by the destruction of the cervical marrow, without its ceasing to perform them. It is also easy to conceive that every other operation capable of suspending or considerably lessening the circulation in a certain extent of the body of an animal, must produce a similar effect, and afford an equal power of attacking without any immediately fatal effects, a portion of the spinal marrow, the destruction of which would have been mortal without that preliminary.

This is what is obtained by the effect itself of destruction of the marrow. This destruction has two effects on the circulation: By the one it weakens the general circulation, by depriving the heart of the contingent powers it received from the marrow destroyed; by the other, without entirely arresting the circulation in the dead parts, it there diminishes it to a very great degree, so as to be to a certain point, equivalent to ligatures on the arteries of the parts.

But this effect does not plainly appear for many minutes after the destruction of the marrow. Thence it happens that the destruction of a first portion of the spinal marrow affords the means of destroying a second, this of a third, and so on. For instance, when, in beheading a rabbit of any age whatever, we are enabled to destroy the cervical marrow, the destruction of this marrow will, in a certain number of minutes, enable us to destroy the quarter of the dorsal marrow, and by continuing to operate thus at intervals on equal lengths of this same marrow, we at last succeed in destroying it entirely without arresting the circulation, which is then kept up by the lumbar marrow only.

We may gather from what we have said, that in rabbits any portion of the spinal marrow furnishes the heart with force sufficient to keep up the circulation in all the parts that correspond with this portion, and that consequently, if we were to cut a rabbit transversely, into several pieces, each piece might be made to live although insolated, if the lungs and heart, necessary to the formation and circulation of the arterial blood, could be included in the piece. But these can only form a part of the thorax; we might therefore very easily support life in the thorax alone, after having separated it from the rest of the body, and prevented hemorrhage by proper ligatures, and this would be the case in rabbits of thirty days and upwards.

Such are the principle results of M. Legallois's investigations. These results, which have a mutual dependance on each other, are founded on direct experiments, performed with a degree of accuracy hitherto unknown in physiology. The author, say the committee, has repeated them in our presence; we have spent three sittings, each of several hours, in examining them, and to avoid all precipitation, as well as to give ourselves time to weigh all the facts at leisure, we have suffered the interval of a week to pass between each session.

All the experiments which were repeated before the committee are ranked, in the report, under two heads. One of which contains those that go to prove that the *primum mobile* of all the inspiratory motions resides in that part of the medulla ablongata which gives origin to the nerves of the eighth pair. The other contains those whose object is to shew that the principle of the powers of the heart is in the spinal marrow. As these details, however interesting, are too long to be inserted here, we shall only say that the author has made similar experiments on warm blooded and cold blooded animals, and it is astonishing to observe the uniformity which experiments made on animals so different, offer in the results, even with regard to the principle of the inspiratory motions. It is well known, that it was particularly after experiments made on frogs that the followers of Haller, and among others Fontana had maintained that the motions of the heart were independant of the nervous power. But experiments repeated with the greatest care on the same animals before the committee, demonstrated at once the inacuracy of this opinion, and of the experiments on which it was founded. The gentlemen of the committee conclude the recital of the experiments repeated before them, by declaring that they completely confirmed all the results above mentioned, and by adopting all the conclusions which the author has deduced from them.

They then continue in these words:

That we may here confine ourselves to the principle points, we will say what we consider as demonstrated.

1st. That the principle of all inspiratory motions has its seat in that part of the *medulla ablongata* which gives origin to the nerves of the eighth pair.

2d. That the principle which animates each part of the body resides in that part of the spinal marrow whence arise the nerves of that part.

3d. That it is, in like manner the spinal marrow which furnishes the principle of life and action; but in this the spinal marrow is throughout concerned, and not a particular portion of it.

4th. That the great sympathetic has its origin in the spinal marrow, and that the particular function of this nerve is to bring all the parts to which it is distributed under the immediate influence of all the nervous power.

These results easily solve all the difficulties that have sprung up since the days of Haller, on the causes of the motions of the heart. The principle of these were, to explain for what purpose the heart receives nerves, why it is submitted to the empire of the passions; and why the circulation continues in acephalous animals and such as have been beheaded. No explication hitherto given was sufficient to reconcile all these points. But at present we can easily conceive why the heart receives nerves; and why it is so evidently subject to the empire of the passions, since it is animated by the whole of the spinal marrow. We may also see that when the circulation continues in acephalous animals, &c. it is because the motions of the heart do not depend on the brain, or at most depend on it but secondarily.

We ought to remark that this last point, on which M. Legallois has thrown so much light, presents nothing but confusion and error in the authors of the Hallerian, as well as of the new school. None of them have distinguished the motions of the heart that take place after decapitation, from those that are observed after the excision of this organ, or after the destruction of the spinal marrow.

The committee next proceed to testify that M. Legallois is indebted to his own industry and zeal for his success in these researches. They then conclude in these terms:

In a word, it appears to us that we may say of the different authors who have considered the matters that M. Legallois has treated, what M. La Place so justly said on a like occasion. "One may find among them many facts, but these are almost always blended with numberless errors, and the discovery of the truth belongs only to him who separates it from the mass, and establishes it on a solid basis, by calculation or by observations."

The opinion of your committee is, that M. Legallois by the service he has done to physiology, merits all the encouragement the class can afford him; and that his researches are of more importance to this branch of knowledge than any that have been made since the days of Haller.

(Signed,)

DE HUMBOLT,
HALLE,
PERCY.

The class approves of the report, and admits the conclusions drawn in it.

The class moreover decrees that the report shall be printed in the records of the class, and that a committee shall consult with M. Legallois with regard to the expense incurred by him for the experiments already made, and the means of continuing them.

G. CUVIER,
Perpetual Secretary.

Hints on the subject of Animal Secretions.

BY EVERARD HOME, ESQ. F. R. S.

Communicated by the Society for the improvement of Animal Chymistry.*

From Philosophical Transactions for 1809, *part II.*

THE brilliant discoveries of Mr. Davy on the powers of electricity in producing chymical changes, suggested to me the idea that the animal secretions may be produced by the same means.

To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chymistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this society; the object of which is the improvement of animal

---

* Dr. Wollaston's observations inserted in the Philosophical Magazine, were published after this paper had been laid before the society.

I was led to the present investigation, while preparing my lectures on the Hunterian Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. William Brande to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir Joseph Banks, and stated that I should bring them forward in my lectures. At that time Dr. Young's Syllabus was not published, and Dr. Wollaston's opinions were unknown to me.

Dr. Berzelius, professor of chymistry at Stockholm, published a work on Animal Chymistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of this opinion, the following experiment is adduced.

"Trace all the nerves leading to any secretory organ in a living animal, and divide them, being careful to injure the blood vessels and the structure of the organ itself, as little as may be: notwithstanding the continued circulation of the blood, the organ will as chymistry. Their intimate acquaintance with these branches of science renders them peculiarly fitted for such an undertaking.

It is one of the most important subjects to which Mr. Davy's discoveries can be applied, and he has given it the consideration it deserves.

The Voltaic battery is met with in the torpedo and electrical eel: and although it is given only as a means of catching their prey, and defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts: one, that a Voltaic battery can be formed in a living animal; the other, that nerves are essentially necessary for its management; for, in these fish, the nerves connected with the electrical organs exceed those that go to all the other parts of the fish in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a Voltaic battery of high power; but their structure appears to Mr. Davy to adapt them to receive and preserve a small electrical power.

That the nerves arranged with muscles, so as to form a Voltaic battery, have a power of accumulating and communicating electricity, is proved by the well-known experiment of taking the two hind legs of a vivacious frog, immediately after they are cut off, laying bare the crural nerves, applying one of these to the exposed muscles of the other limb, and then when the circle is completed, by raising the other crural

---

little secrete its usual fluid, as an eye deprived of its nerve can see, or a muscle whose nerve has been divided can move. We may therefore easily conceive, that any trifling alteration in the nerves of a gland may materially affect its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting when we discover the insufficiency of all other explanation. Dr. Berzelius's work was shown to me by Mr. Davy while this paper was in the press.

nerve with a glass rod and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs. The ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles, and the course of the nerves of the viscera which keep up a connexion among themselves in so many different ways.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. W. Brande, who has begun his career in animal chymistry with so much success, made the following experiments, in the suggestion of which Mr. Davy afforded him every assistance.

Experiment I. Middle of January, 1809.

The conductors from twenty-four four-inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been withdrawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire was of a deep red colour and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment II. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the meso-colon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three-inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of an hour; a third ligature was then tied in the centre of the detached vein, in order to cut off the connexion between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long-continued electrical action, the serum only was employed in the following experiments.

Experiment III. 10th of March, 1809.

The conductors from one hundred and twenty four-inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved;

at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

EXPERIMENT IV. 14th of April, 1809.

Was undertaken to ascertain the effect of a low power: a battery was employed, consisting of twelve four-inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole, in five minutes, the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in length was seen floating in the fluid, and adhering to the same wire.

EXPERIMENT V. 6th of May, 1809.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three-inch plates very *weakly* charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing, it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid, (super salts.)

By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form; with a less degree, it is separated in a fluid form.

From these facts, the following queries are proposed:

1st. That such decomposition of the blood by electricity, may be as near an approach to secretion as could be expected to be produced by the artificial means at present in our power.

2d. That a weaker power of electricity, than any that can be readily kept up by art, may be capable of separating from the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose; and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed.

5th. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6th. As albumen becomes visibly coagulated, by the effect produced from twelve four-inch double plates of copper and iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chymical test of electricity, whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.

Kelso on the use of Cotton in Burns.

SIR,

SINCE facts are the only true tests of the efficacy of any remedy, I beg leave to submit the following case for your consideration. In perusing the first number of the Lycæum, my attention was attracted by Dr. Dallam's paper on the use of cotton in scalds and burns. Having long wished for a remedy that would procure relief from the severity of pain in such cases, it appeared to me that the cotton was the remedy so much to be desired. Never having found a remedy that would satisfy me on this point, and having frequently been disappointed in the spt. terebinth. (the celebrated cure of Dr. Kentish) which generally was found to aggravate the pain on its first application; I determined to resort to the use of the cotton as soon as a suitable case should present itself. As our patients flatter themselves with the expectation of immediate relief on our arrival, it appeared to me that even the temporary mitigation of pain would be no small acquisition.

On the 16th of April, 1812, I was requested to visit a child in the country, between two and three years old, that had been accidentally scalded, by its mother having spilt a pot of boiling water over a considerable part of its body.—When I arrived at the house I found the child in great pain, and upon examination, the water had extended from the os occipitis to the os calcis; it had extended over the deltoid, latissimus dorsi and glutæi muscles; the whole put on a high scarlet appearance; a slight vesication arose on the tip of the left shoulder and on the left glutæus muscle. I lost no time in applying the cotton as directed by Dr. Dallam; and in about twenty or thirty minutes was agreeably surprised to find the little sufferer fall into a sound sleep. I remained in the house an hour after the child began to sleep, and then was compelled by other business to leave it, with particular orders to continue the cotton. The next day it was not in my power to visit my little patient, but I was informed that it awoke perfectly free from pain, and returned to its usual habits of playfulness. A slight covering was still kept to the parts to keep them from rubbing or getting any way injured, as the skin remained tender. On the next day I paid it a second visit, and upon examination I found that the inflammation had entirely subsided, the scarlet colour having disappeared. Nothing now seemed to be necessary except to keep the part covered from the air.

It appears to me, from the extent and great degree of inflammation first produced, that had it not been prevented by the cotton, one general vesication would have been the consequence. As the application in such cases is a new remedy, it would be highly gratifying to me should this case, together with Dr. Dallam's observations, tend to excite others to similar trials.

D'r sir, your ob't humble serv't

JOS. KELSO.

Dr. Potter. *Harrisburgh*, 4*th May*, 1812.

……

Interesting particulars concerning a new and undescribed species of nettle (urtica) which is capable of being manufactured into yarn, thread, cordage and cloth. In a letter from Samuel L. Mitchell, M. D. dated Washington, Jan. 27, 1812, to the editor.

Governor Tomkins to Dr. Mitchill.

NEW-YORK, NOV. 16th, 1811.

SIR,

MR. CHARLES WHITLOW, of this island, has discovered a weed which he thinks will be a complete substitute for hemp and flax. He avers that it can be raised in larger quantity upon the acre than either of them, that it is not any species of hemp now known, and that it has no botanical name. I enclose you a few inches of the top of a stem before breaking, and the flax of a whole stem, broken with the fingers, and a short cut from a skein of thread spun of the flaxen part before it was either rotted or bleached.

Dr. C. W. Eddy to Samuel L. Mitchill.

NEW-YORK, Nov. 21st, 1811.

I duly received your note of the 19th, this morning, and being unfortunate in not having a knowledge of the plant from which the hempen material is procured, I took a ride his afternoon to see Mr. Whitlow, from whom I learned that it was as yet a secret, and that it was unknown to Walter, Michaux, Persoon and all others who have at any time written on or described the plants of this country; accordingly I desisted from all farther enquiries on that point. He however, shewed me some large skeins of *thread*, and also a considerable quantity of tow, made of it, adding that the woman who spun it, declared that should it be properly rotted and bleached she could make it produce thread of a quality one half finer. I was also informed by Mr. Whitlow that the thread weighed at the rate of *ten dollars per pound.*

Opinion in reply.

WASHINGTON, Dec. 20th, 1811.

This plant is a native of New-York. It is a hardy perennial. It is believed by Muhlenberg to be a species not hitherto described by any botanist. It belongs to the genus of URTICA, which abounds in plants having stringy and fibrous coats, applicable to the manufacture of thread, ropes and even cloth. The *nettle* now under consideration is proposed to be named in honour of the discoverer, *Urtica Whitlawi.* Its character, though sufficiently distinct, is however such as to place it nearer to the *Urtica Divaricata* and the *U. Canadensis* than to any others. If this plant will afford a good crop of *cortical* or *fibrous* matter, its durability will give it a preference to such annuals as flax and hemp, by saving the yearly expense of tillage and seed.

The following is its Botanical character.

Caule—5 angulari, simplici, orgyali, urente.

Foliis—Alternis, cordato, ovatis, acutis, serratis, trinerviis, punctatis, petiolatis, supremis oppositis.

Stipula—Bifida.

Paniculis--Pedunculatis, axillaribus, dichotomiis, hirsutis, petiolo longioribus masculis, et terminalibus fœmineis.

Capsula—Orbiculari, compressa, mucronata, proxima *divaricatae* et *Canadensi.*

Radix.—Perennis.*

For which the scientific world is indebted to Muhlenberg.

Certificates of its value.

At the request of his excellency governor Tomkins and his honour the mayor of New-York, we, the subscribers, manufacturers of linen, flax and cotton, met Mr. Charles Whitlow, botanical collector, for the purpose of examining a perennial plant, discovered by him, which he exhibited to us in its various stages. Having particularly examined the same, we are unanimously of opinion that it is far superior to any hemp or flax we have seen, either in Europe or America, in the quantity it produces from a single stem. The superior strength, beauty and fineness of its texture, and the experiment that has been made proves that it will produce from twenty to twenty-five per cent more from the hackle than any flax or hemp known to us. And we are fully persuaded that it will become a great benefit as a staple article to any country that will encourage its cultivation.

---

* For the use of those who may not readily comprehend Latin technical terms, we subjoin an English translation:

Stem.—Simple five angled, irritating by pungent stimuli.

Leaves.-Heart-oval, acute, serrated, three nerved, dotted, placed on alternate footstalks below, though opposite above.

Stipules.—Bicleft.

Flowers.---Axillary pannicles on bifurcated pubescent stems longer in the males than the footstalks of the leaves. Female flowers terminal.

Capsule.--Orbicular, compressed, daggered, resembling that of the divaricated and Canada species.

Root.---Perennial.---*(Editor.)*

Dated, New-York, this 14th Nov. 1811.

JOHN CRITCHTON,
F. STEVENSON,
ROB'T MITCHELL,
ROB'T THOMSON.
ZAC. PEACOCK.

[*True Copy.*]

The plant above mentioned, with the flax, tow and thread produced from the same, having been examined by us, we are of opinion that its cultivation will be highly beneficial to any country that will encourage it.

THO'S C. BUTLER,
CORNELIUS BENSCOWFER,
BURNELL BROWN.

[*True Copy.*]

New-York, Nov. 16*th*, 1811.

We have seen and examined a plant in the hands of Charles Whitlow, which so far as we are capable of judging from viewing it in the different stages of its manufacture, as presented to us by Mr. Whitlow, appears well calculated to answer all the purposes of hemp and flax, and merits cultivation assiduously, as it promises to be highly beneficial to every country, which will encourage its cultivation and manufacture.

JOHN KEMP, } Professors of Co-
P. WILSON, } lumbia college.

Attested, FRED. PURSH.

Observations on a new plant of the hemp and flax species; lately found by Mr. Charles Whitlow.

This article appears to be particularly valuable on account of the harl being inclosed in the body of the substance, and not like hemp or flax, merely covering a pithy or wooden substance.

As the pithy or wooden substance in hemp or flax occupies at least from three-fourths to seven-eighths of its body, I think myself qualified to assert that this plant will produce from three to four times more than the present species of flax or hemp now cultivated. The plant appears to be a species of flax. Its productive growth recommends it as likely to be a valuable article of agriculture. The harl is really strong, it spins easy and works readily and is well adapted for machinery. On the whole I think it a discovery which entitles Mr. Whitlow to a compensation for his trouble and reward for his investigation.*

JOHN DALTON.

New-York, 15*th Nov.* 1811.

---

* The discovery of this article and the economical purposes to which it may be applied, ought to act as a strong inducement to the lovers of knowledge, to cultivate an acquaintance with natural history. Our country abounds in animals, vegetables and minerals that may be brought into use and render life more comfortable, as well as increase the ratio of population. Millions of acres lie waste around us, only because we are without a vegetable to suit the habit or capacity of the soil. The barren mountain may by future discoveries be made a garden; and the noisome marsh that now evolves only miasmata deleterious to animal life, may be made to blossom as the fruitful valley.

The hemp can scarcely find a spot in our Atlantic States to satisfy its rapacious demands, while plants perhaps of equal value, may be found to flourish on our stony fields and grounds, rendered by long use incapable of the usual crops.

By a knowledge of botany we identify plants, give intelligent descriptions of them, and are led to form the strongest conjectures both as to their medicinal and economical uses. Thus we see that from a slight resemblance in character Linnæus has denominated one of the species of urtica *cannabina*, and that Jussieu has placed the *cannabis* and *urtica* in the same class and order of plants.—*Editor.*

Hopkins on the poisonous effects of Strammonium.

SIR,

AGREEABLY to promise I send you some account of the poisonous effects of Strammonium or Thornapple. The following case came under my notice about fifteen years ago, and has taught me a lesson that has been useful to me:

A girl about eight years old, remarkable for the enjoyment of good health, had left the house perfectly well: on her return she threw herself on the bed and presently became stupid, to all appearance insensible, and finally delirious. Upon examination I fond the pulse irregular, and an extraordinary dilatation of the pupils was remarkable. As the family could give me no account of the cause of the symptoms, I supposed the disease to have arisen from worms; and immediately exhibited ten grains of Calomel and a gill of Castor Oil which were swallowed with great difficulty, on account of the great want of sensibility and an aversion to all exertion or motion. I had the patient then immersed in warm water, and having waited two hours in vain, for the operation of the medicine, I gave her ten grains of Calomel, with two of Jalap, and two of Gamboge. To these medicine I added a table spoon full of Castor oil every half hour, and followed the doses by stimulating injections to quicken the operation of the rest.

In about five hours the medicine began to operate; and to our surprise more than a table spoon full of the seeds of the Strammonium were discharged at the first evacuation, and smaller portions at several successive discharges. They came away just as they had been chewed, partly masticated, but unaltered by the action of the gastric juice. The child was immediately cured, and the case brought to my mind the truth of the old adage, that "the cause being known the disease is half cured;" for my ignorance of the cause of the malady lost to me the benefit of a more speedy cure, the exhibition of a brisk emetic. As it happened, the effects of the active medicine and my having remained with the patient saved her life. My principal reason for wishing to record this case is, that should such other cases occur (as no doubt they frequently do) other practitioners may avail themselves of my misconstruction of the cause and use a more prompt and energetic mode of relief.

About a year after I attended the above case I was sent for in haste by a family that was in great consternation on account of three of their children. They had gone out of the house in good health, and when they returned the father and mother were astonished at the uncommon appearance of their eyes, the pupils being widely dilated. As soon as I saw them and witnessed their behaviour and appearance, I had no difficulty in ascribing their complaint to the right cause. On interrogating the children they readily acknowledged that they had been employed in plucking the leaves of the Strammonium, and that after bruising them they had rubbed them over each others faces.

Since I drew up the above cases, I was requested to visit a child about three years old. It had plucked a pod of the Thornapple and had eaten about two tea spoons full of the seeds. It soon became quite stupid and insensible. The family unsuspicious of danger, neglected to procure medical assistance till the morning after the afternoon on which it had swallowed the seeds; and the child died about an hour after I reached the house.

Let it not be inferred from these deleterious effects of the Jamestown weed that it may not be, when properly prepared and judiciously employed, a valuable acquisition to the Materia Medica. The best medicines in too large doses are poisons and the most virulent poison in proper doses is often the best medicine. We have so many proofs of the virtues of this medicine both externally and internally, that it would be superfluous to dilate on the subject.

With best wishes your friend.

RICHARD HOPKINS.

Observations on Albumen, and some other Animal Fluids; with Remarks on their analysis by electrochymical decomposition.

BY WILLIAM BRANDE, F. R. S.

Communicated by the Society for the Improvement of Animal Chymistry.*

I. *Observations on Mucus, and on the composition of liquid Albumen.*

THE results obtained from the chymical analysis of the intervertebral fluid of the spalus maximus, an account of which is annexed to Mr. Home's paper "On the nature of the intervertebral substance in fish and quadrupeds,"† led me to undertake a series of experiments on mucus, in order to examine the properties of that secretion in its pure state, and to ascertain how far it might be capable of conversion into modifications of gelatine and albumen.

1. Saliva was the first source of mucus to which I directed my attention.

In order to separate the albumen, which Dr. Bostock's analysis has shown it to contain,‡ it was agitated for a short time with an equal quantity of pure water; the solution was then boiled and filtered. I considered the clear fluid, which had passed the filter, as a solution of nearly pure mucus; but found, on applying to it the tests of nitrate of silver, and acetate of lead, that it still contained a very considerable proportion of saline matter. The precipitate consisted of muriate and phosphate of silver and lead, in combination with a little animal matter, the odour of which was perceptible on exposing it to heat after it had been washed and dried.

One thousand grains of saliva afforded, by careful evaporation in a water bath, a residuum weighing one hundred and eighty grains, from which twenty grains of saline matter, consisting of phosphate of lime and muriate of soda, were obtained by incineration.

---

* Philosophical transactions for 1809, p. 373.

† Philos. Trans 1809; or Journal, vol. XXV. p. 214.

‡ Nicholson's Journal, vol. XIV. p. 149.

2. The mucus from the trachea, and that of the oyster were next examined; but here the proportion of saline matter was greater than in the former case, although no traces of albumen could be detected by the usual tests of heat, alcohol and acids.

Finding, therefore, that the re-agents employed to detect mucus* act principally upon the salts which it contains, and not merely upon the secretion itself, it became an object of some importance to find out a method of depriving it of its saline ingredients, by such means as should not affect the mucus. Decomposition by electricity immediately occurred to me, as the most likely means of attaining the object I had in view.

For this purpose I procured three glass cups, each capable of holding rather more than a measured half ounce of water; one of these was filled with a mixture of equal parts of saliva and pure water; this was connected with the other two, containing pure water, by filaments of moistened cotton. The water in one of the cups was rendered positive, that in the other negative, by a Voltaic battery of one hundred and twenty four inch double plates, charged with a solution of nitro-muriatic acid, in the proportion of one part of the mixed acid to thirty parts of water.† By continuing this process, I hoped to decompose the saline ingredients of the saliva, to collect the acid matter in the positive, and the alkaline matter in the negative cup, and thus to leave the mucus and albumen in the centre vessel (free from the salts which they contain in their natural state,) and to have separated them by boiling distilled water, which would then have afforded a solution of pure mucus.

---

* Nitrate of silver and acetate of lead. Vide Thomson's system of Chymistry, vol. V. p. 500, third edition; and Nicholson's Journal XI...251.

† It was conceived that this electrical power, though sufficient for the decomposition of the salts, would not materially affect the animal matter.

When the action of the battery had been continued for about ten minutes, a considerable quantity of a white substance surrounded, and adhered to the cotton on the negative side of the circuit, whereas on the positive side no such effect had taken place.

I could not at first account for this appearance, conceiving that, if it depended on the coagulation of albumen held in solution in the saliva, it would have taken place at the positive pole, in consequence of the acid there separated.

To ascertain this point, an experiment was made on the albumen of an egg.

When the conductors from the same battery were brought within two inches of each other in this fluid, an immediate and rapid coagulation took place at the negative wire, while only a thin film of albumen collected at the positive wire, where its appearance was readily accounted for, by the separation of a little acid, which, reacting on the albumen, would render it solid; but the cause of the abundant coagulation at the negative pole was not so obvious.

This result I mentioned to Mr. Davy, who immediately offered an explanation of it, by supposing the fluidity of albumen to depend upon the presence of alkaline matter, the separation of which, at the negative pole, would cause it to assume a solid form. I had only to follow up this idea, and shall proceed to state the principal experiments which were undertaken to establish so probable an opinion.*

1. When coagulated albumen, cut into small pieces, is boiled in distilled water, it imparts a viscidity to that fluid, showing that something is retained in solution.

Two hundred grains of the coagulated albumen of an egg were repeatedly washed and triturated in four ounces of distilled water, which was afterwards separated by a filter, and evaporated to about one fourth of its original bulk. It was then examined by the usual tests, and was found evidently alkaline; it converted the yellow of turmeric to a pale brown, and restored the blue color to litmus paper, reddened by vinegar; but it did not appear to effervesce on the addition of a diluted acid.

---

* On referring afterwards to Dr. Thompson's system of Chymistry, (vol. V. p. 491) I find, that a very similar explanation of the coagulation of albumen has been offered by that author, which the following experiments will likewise confirm.

On evaporating this alkaline fluid to dryness, by a gentle heat, a viscid substance, soluble in water, was obtained. This solution was rendered slightly turbid by an acid; and by the application of electricity, from sixty four-inch double plates, a copious coagulation took place at the negative pole.

So that water, in which the coagulated white of egg has been boiled, is in fact an extremely dilute alkaline solution of albumen.

This enables us also to explain why albumen becomes coagulated simply by heat.

When the coagulated white of egg is cut into pieces, a small quantity of a brown viscous fluid gradually separates from it, as has been observed by Dr. Bostock in his paper on the primary animal fluids.* This I find to consist principally of an alkaline solution of albumen. It reddens turmeric, and coagulates abundantly on the application of negative electricity.

It appears, therefore, that the white of an egg, in its fluid state, is a compound of albumen with alkali and water; that when heat is applied to it, the affinities existing between these bodies are modified; that the alkali, before in chymical combination with the albumen, is transferred to the water; and that this separation causes the coagulation of the albumen: the aqueous alkaline solution, which is thus formed, reacts upon the coagulated albumen, of which it dissolves a small portion, and then appears in the form of the brown viscous fluid already noticed.

The coagulation of albumen by alcohol and by acids may be explained by a reference to the principles already laid down.

---

* Nicholson's Journal, vol. xi—246.

1. Five hundred grains of the white of egg were agitated with two ounces of pure alcohol; an immediate coagulation resulted, which was rendered more perfect by the application of a very gentle heat. The liquor was separated from the coagulum by filtration, and evaporated to half its bulk; when the usual tests were now applied, alkaline matter was abundantly indicated.

In this instance then, the albumen, in passing from the liquid to the solid state, gives its alkali to the alcohol.*

2. When acids are applied to albumen, these effect its coagulation from the same cause: they render it more rapidly and more perfectly solid, on account of their superior affinity for the alkali.

The following experiments were instituted with a view to ascertain the nature and quantity of the alkaline matter which exists in liquid albumen.

1. Five hundred grains of the liquid white of egg were mixed with two ounces of distilled water, and exposed for half an hour to a temperature of 212°. The fluid was then separated by a filter, and the coagulated albumen cut into small pieces, and repeatedly washed with boiling distilled water. The filtrated fluid was evaporated to half an ounce by measure; it had a saline taste, it was somewhat turbid, and slightly alkaline; on cooling, it gradually deposited a few flakes of albumen: it was electrified positively in a small glass cup, connected by washed cotton to another similar vessel containing a little distilled water, negatively electrified by one hundred four-inch plates, charged with a solution of nitro-muriatic acid of the same strength as that employed in a former experiment, fresh portions of water being occasionally added in order to compensate for the loss by its decomposition.

When the electrization had been carried on in this way for one hour the cups were removed, and their contents examined.

---

* When albumen is coagulated by alcohol, it does not become so perfectly solid as in most other instances, because the separation effected by the relative affinities is not so complete.

The fluid in the negatively electrified cup acted rapidly on turmeric, rendering it deep brown. On evaporation and subsequent exposure to a low red heat, it afforded a residuum weighing 5·5 grains, which had the properties of soda, in a state approaching to purity.

The positive cup contained a little coagulated albumen, and an acid which was principally, if not entirely the muriatic, was held in solution by the water: it gave a very copious precipitate with nitrate of silver, which became speedily black on exposure to light. When saturated with carbonate of soda, and evaporated, it afforded a salt in small cubic chrystals, from which the fumes of muriatic acid were developed by the action of the sulphuric.

This experiment shows, that, exclusive of soda in an uncombined state, fluid albumen contains some muriate of soda.* We learn from the experiments of Mr. Hatchett, that minute quantities of other saline bodies are likewise present.†

In the foregoing experiments, I had generally employed from sixty to three hundred four-inch double plates of copper and zinc, but in subsequent researches, made with a view of ascertaining the action of lower powers, the effects of which I shall afterwards relate, I find that a battery of twenty

---

[*] May not a submuriate of soda exist in fluid albumen?

[†] After the destructive distillation of coagulated, dry, semitransparent albumen, there remained "a spongy coal of very difficult incineration; as towards the end of the process it appeared vitrified, and glazed with a melted saline coat, which was, however, easily dissolved by water. The residuum was again exposed to a long continued red heat, and again treated with water, till, at length, a few scarcely visible particles remained, which, as far as such small quantity would permit to be ascertained, proved to be phosphate of lime. The portion dissolved by water (which was by much the most considerable) consisted principally of carbonate, mixed with a small quantity of phosphate of soda.

"Five hundred grains of dry albumen afforded 74·50 grains of coal, of which 11·25 were saline matter."

See "Chymical experiments on Zoophytes, with some observations on Component Parts of Membrane." Phil. Trans. 1800.

four three-inch double plates is sufficient to effect a perfect coagulation at the negative pole, even where the albumen is diluted with so large a quantity of water, as not to be detected by the usual tests.

II. *Observations on the Composition of some animal Fluids containing Albumen.*

Finding, from the experiments detailed in the preceding section, that albumen may exist in such states of combination, as not to be detected by the usual tests, but separable by electrical decomposition, I was induced to apply this mode of analysis to the examination of animal fluids in general.

1. *Saliva.*

When saliva is boiled in water, a few flakes of coagulated albumen are deposited; but this is by no means the whole quantity of albumen contained in the secretion, for on applying the test of negative electricity to the filtered fluid obtained after the separation of the albumen by heat, a copious coagulation and separation of alkali are produced at the negative pole. A large portion of albumen may therefore exist in a fluid, incapable of separation by heat, and in the present instance not to be detected even by acids, these re-agents producing no effect on the filtered solution just alluded to.

2. *Mucus of the Oyster.*

The solution of mucus obtained by agitating oysters in water exhibits to the usual tests no traces of albumen; but when acted upon by electricity from the Voltaic battery, a considerable and rapid coagulation takes place at the negative pole, by electrified wire.

3. *Mucus of the Trachea, &c.*

The other varieties of mucus, as from the trachea, the nose, &c. agree with the former, in affording abundance of albumen by electric decomposition; whereas scarcely any traces of this substance can be detected by the tests of acids, heat, or alcohol.

In these experiments, alkaline matter was always evolved at the negative, and acid at the positive wire. Minute researches, made with a view of ascertaining the nature of the alkaline and acid matter thus evolved, showed the former to consist of soda, with traces of lime; the latter of muriatic acid, with traces of phosphoric acid in the cases of saliva, and mucus of the trachea and the nose: the mucus of the oyster afforded only soda and muriatic acid.

On examining the proportions of alkali and acid, the former seemed always to predominate, although in the original fluids no traces of uncombined alkali (as in the white of egg) are to be detected.

These results lead to new ideas respecting the composition of mucus: Is it a peculiar combination of muriate of soda and albumen? or may it not be a compound of soda and albumen, in which the alkali is not separable by the usual modes of analysis, but which yields to the superior decomposing energy of electricity?

4. *Bile.*

An immediate coagulation took place in this secretion, at the negative conductor, the albumen being tinged throughout of a green colour, arising from the colouring matter at the same time separated.

The relative proportion of albumen, separable by electricity from different specimens of ox-bile, was found to be liable to considerable variation, so that a detailed analysis of this fluid cannot be generally depended upon. I have found the albumen in bile to vary in quantity from 0·5 to three per cent.; and it is somewhat remarkable, that where there is a small quantity of albumen, there likewise the proportion of the resinous matter of bile is relatively small.

The electrochymical decomposition of this fluid affords, beside the results just mentioned, a considerable quantity of soda at the negative pole; and at the positive pole, a mixture of muriatic and phosphoric acids.

5. *Milk.*

In this fluid, the separation of albuminous matter at the negative pole is equally evident, though not so rapid, as in most other cases. The conductors from sixty four-inch double plates, highly charged, and immersed within four inches of each other in three ounces of cow's milk, during one hour, produced the appearance of curds and whey, the principle part of the curd being collected in the neighbourhood of the negative wire, and but little at the positive wire. When this experiment was so conducted, as to collect the products in separate vessels, the predominating ingredients in the contents of the negative cup were soda and traces of lime; and in the positively electrified vessel, a mixture of muriatic and phosphoric acids.

After such decomposition of milk, the serum still affords sugar of milk.

6. *The Liquor of the Amnios.*

An opportunity having offered of examining this secretion, from the human subject, in its pure and fresh state, I shall mention the general results of its analysis.

The liquor of the amnios is almost perfectly transparent, but on exposure to air becomes gradually turbid, and deposits a white flaky matter. It renders tincture of violets green, and while perfectly fresh does not affect litmus; but sulphurated hydrogen is soon evolved from it, and then it slightly reddens litmus. When heated, it becomes turbid, and lets fall flakes of coagulated albumen. Acids render it slightly turbid from the same cause.

Alkalis produce no change, unless when added in considerable excess: the odour of ammonia is then perceptible.

Electrical analysis afforded albumen and soda at the negative pole, and muriatic acid at the positive pole. Hence we learn, that the liquor of the amnios has the properties of a dilute solution of liquid albumen.*

---

* The difference in the results of the analysis given in the text,

7. *Pus.*

In the pus of a healthy sore, coagulation took place at both poles; most abundantly, however, at the negative pole. A slight degree of putrefaction having commenced in the pus which was examined, I did not pay particular attention to the other products of the experiment.

In concluding this section, it may be proper to remark, that the decomposition of liquid albumen by Voltaic electricity takes place in different ways, according to the power employed. With a comparatively high electrical power, the coagulation goes on rapidly at the negative pole, and only very slowly at the positive pole; whereas, with an extremely low power, the coagulation is comparatively rapid at the positive surface, an alkaline solution of albumen surrounding the negative pole. Thus, when the conductors from twenty four four-inch double plates, highly charged, were brought within half an inch of each other, in a dilute solution of albumen (consisting of one part of albumen to six of water,) the coagulation was considerably more abundant at the negative than at the positive pole: but when the conductors were removed from each other to a distance of eight inches, or when they remained at half an inch, being connected with a battery of six four-inch double plates only, the coagulation was only perceptible at the positive pole, in consequence of the acid there collected. Hence we may infer, that a rapid abstraction of alkali is necessary to the perfect coagulation of albumen, since, in the cases above alluded to, the albumen remains in solution.

---

and that of Vauquelin and Buniva, most probably arises from the liquor of the amnios examined by those chymists not having been perfectly recent, and perhaps mixed with other secretions.--- See *Annales de Chimie,* XXXIII. p. 279.

From the American Medical and Philosophical Register.

A Memoir upon the *Organs of Absorption* in Mammiferous Animals, read at the Institute, on the 7th of August, 1809, by M. Magendie, Doctor of Medicine of the Faculty of Paris, Professor of Physiology, &c. The experiments conducted by Doctors Magendie and Delile. Communicated to Dr. David Hosack, by A. R. Delile, M. D. of the Institute of Egypt, &c.

AMONG the facts which I had the honour to report to the class, in a memoir upon the upas tieutê, the nux vomica, and the bean of St. Ignace, there is one which appeared to me worthy of more particular attention; I mean the readiness with which those poisonous matters are absorbed and introduced into the sanguiferous system. It must be recollected, that it scarcely requires twenty seconds to convey these poisons from the peritóneal cavity to the spinal marrow.

The generally received ideas relative to the organs of absorption, do not admit of a doubt that the lymphatic vessels are the agents for conveying these poisons into the sanguiferous system. Thus, in the experiment where the poison was introduced into the middle of the thigh of an animal, there was but one way of explaining its absorption; it must necessarily be admitted that it was taken from the wound by the lymphatic vessels of the parts with which it was in contact; that after being absorbed it was carried by these vessels towards the glands of the groin; that after traversing these bodies, it was conveyed, still by the lymphatic vessels, to the thoracic duct; finally, that it was introduced into the sanguiferous system by the communications which the thoracic duct preserves with the subclavian veins, and principally with those of the left side.

Such ought to have been, and such, in fact, was our opinion, at the time of the publication of the memoir upon the strychnos. Nor were the experiments, of which I am about to give an account, undertaken with a view to discover new facts, but rather to add a degree of certainty to an explanation already admitted; and our labours did not take a particular direction until a great number of facts obliged us to modify our view of this subject. But so rapid an absorption by vessels, whose principal characteristics are weakness and slowness of action; a poisonous substance that so quickly pervades the difficult and winding route of the lymphatic glands, without any alteration therein, were two circumstances that ought, perhaps, to have made us entertain some doubts of the correctness of the received explanation. This explanation, however, is given by so many respectable persons, and is supported by experiments so positive, that even now, when we have many facts to oppose it, we dare not say that it wants exactness, but only, that it is not admissible under every circumstance.

Previous to any detail of our experiments, it will not be useless to relate, in a few words, an opinion which for some time balanced the present prevailing belief relative to the organs of absorption.

This opinion, professed by Boerhaave, Haller, Méckel, Ruysch, Swammerdam, and others, was, that the sanguiferous veins, in common with the lymphatic vessels, possessed an absorbing power.

It is supported by different circumstances of structure, and by some physiological and pathological facts. A series of interesting experiments, undertaken and executed a few years since, at the veterinary school of Alfort, has also strengthened the probability of such absorbent property of the veins, but without producing entire conviction. It is well understood that an opinion established upon the physical structure of the organs, deduced from a sufficient and conclusive number of experiments, and supported by the names of Boerhaave, Haller, and Ruysch, ought not to be easily abandoned. Nor was less required than the anatomical discoveries of the last century, the correct experiments of Hunter and his brother, those of Cruikshank, Mascagni, Desgenettes, and others, to establish the belief that the lymphatic vessels only possess the absorbent faculty.

I will further cite, in support of the general opinion, some very curious experiments lately made by M. Dupuytren.—This physiologist, who has kindly permitted me to report the principal results of his labours, tied the thoracic duct in several horses; some of them died in five or six days, others preserved every appearance of health. We know already by an experiment of Duverney, by some observations on the thoracic duct when obstructed, and above all, by the experiments of Landrin, that the thoracic dust may cease to convey the chyle into the subclavian vein without being followed by the death of the animal; it is true, we also know, that some animals died in consequence of a ligature round the duct; but we are entirely ignorant of the cause of this diversity in the results. M. Dupuytren, by his experiments, has found one very satisfactory. In the animals that died in five or six days, from the ligature round the thoracic duct, he always found it impossible to pass any injection from the inferior part of the duct into the subclavian vein; consequently, it is very probable, that the chyle ceased to be conveyed into the venous system immediately after the application of the ligature. On the contrary, in all the animals who have survived the application of the ligature, it has always been easy to make every kind of liquid pass from the inferior part of the duct to the subclavian veins, by means of the very numerous communications between these two points by the lymphatic vessels, placed alike in the posterior as in the anterior mediastinum.

I have personally assisted M. Dupuytren in opening a horse, the thoracic duct of which he had tied more than six weeks before, and I easily satisfied myself that there existed evident communications between the inferior portion of the duct and the subclavian veins, although this canal was entirely destroyed at the place of the ligature.

I now proceed to the experiments, which I made, for the most part, in concert with M. Delile, to determine, whether the lymphatic vessels are really the only ones by which foreign substances enter into the venous system.

A circumstance that has always thrown some obscurity over the experiments upon absorption, is, the difficulty of demonstrating, with certainty, the passage and presence of absorbed matters, either in the lymphatic or sanguiferous vessels. We have not these inconveniences to fear in employing the upas, or the *nux vomica;* for it is known that two centigrammes of these substances produce effects too remarkable to be mistaken.

Would the ligature round the thoracic duct stop the passage of the poison in the sanguiferous system, and consequently, its effects upon the spinal marrow? This was the first question that we proposed to resolve.

I have applied in a dog a ligature to the thoracic duct, a little before its opening into the left subclavian vein; I afterwards introduced a solution of the *upas* into the cavity of the peritoneum. The effects of the poison were as prompt and marked as if the thoracic canal had not been tied. I have tried the same ligature in other animals; but instead of introducing the poison into the cavity of the peritoneum, I introduced it either into the pleura, or into the stomach, the intestines, or muscles of the thighs, &c. The effects have always been equally rapid and intense, as if the thoracic canal had been free.

Decisive conclusions cannot be drawn from these first essays, for we know that the thoracic canal is not the only point of communication between the lymphatic and venous systems. There is ordinarily on the right side, a second thoracic duct, almost as considerable as that on the left; the large lymphatic vessels often open themselves solitarily into the subclavian veins; and more frequently still the thoracic duct has several mouths into the vein where it terminates.

One of these circumstances was found to occur in the animals subjected to our experiments; and we must have recourse to other trials, from which other results, less equivocal, may be deduced.

Upon a dog who had eaten a large quantity of meat seven hours before, in order that his abdominal lymphatic vessels might be easily perceived, we made an incision through the parietes of the abdomen, and took from thence a coil of the small intestines, upon which we applied two ligatures about five inches apart from each other. The lymphatics, which had their origin in this turn of the intestine, were very white and very apparent from the chyle which they contained. Two ligatures were placed at the distance of one centimetre upon each of these lymphatics; we cut these vessels between the two ligatures; we took great care in this part of our experiment, and assured ourselves, by every possible means, that the curvature of the intestine, taken from the abdomen, had no further communication with the body by the lymphatic vessels. Five arteries and five mesenteric veins ended in the portion of the intestine comprised between the two ligatures; four of these arteries and four of the veins were tied and cut in the same manner as the lymphatics; the two extremities of the curvature of the intestine were cut and entirely separated from the rest of the small intestine. Thus we had a portion of the intestine about five inches in length, not communicating with the rest of the body but by one artery and one mesenteric vein; these two vessels were separated at the distance of four fingers breadth. We even raised the cellular substance, lest some lymphatic vessels might lie there concealed. We had nothing further, by which to obtain a positive result, than to inject a small quantity of the *upas tieute* into the cavity of the intestinal curvature. This was also done with proper precautions to prevent the escape of the injected liquor. The curvature of the intestine, enveloped in fine linen, was replaced in the abdomen; it was then precisely one o'clock. To our great astonishment, about six minutes after, the general effects of the poison diffused themselves with their usual intensity, and in such a manner that every thing proceeded as if the curvature of the intestine had been in its natural state.

The animal being dead, we examined the parts; no ligature was displaced, nothing could make us suppose that the poison had passed into the abdominal cavity.

This experiment, repeated several times without any modification in the result, appeared to us most conclusive; it proves, at least, as far as can be proved in physiology, that the lacteal vessels are not the exclusive organs of intestinal absorption.

This kind of absorption, different from that of the lymphatic, may be peculiar to the intestines; it would be important to know if it could be discovered to exist in the other parts.

We separated from its body, the thigh of a dog, previously rendered comatose by opium, (in order to spare it the pain of a difficult experiment;) this separation was so made, that the thigh still communicated with the trunk by the *crural* artery and vein. We took, with regard to these two vessels, the same precautions as for the mesenteric vein and artery in the preceeding experiments; that is, we isolated them on an extent of four centimetres, and raised their cellular *coat*, least it should conceal some lymphatic vessels; we then inserted two grains of the poison in the foot, and waited the effects. They manifested themselves with as much promptitude and energy, as if the thigh had not been separated from the body; insomuch, that the first signs of the action of the upas appeared before the fourth minute, and the animal died before the tenth.

It may be objected, that notwithstanding all the precautions taken, the arterial and venous parietes still contained some lymphatics, and that these vessels were sufficient to give passage to the poison. It were easy to refute this objection.

I repeated upon another dog, the preceding experiments, with this modification, that I introduced into the crural artery a small quill, upon which I fixed this vessel by two ligatures; the artery was afterwards cut circularly between these two bands. I did the same to the crural vein; so that there was no longer any communication between the thigh and rest of the body, unless by the arterial blood which comes to the thigh, and by the venous blood which returns to the trunk. The poison introduced into the foot produced its general effects in the ordinary time, that is, in about four minutes. It may be deduced, I believe, from these different experiments, that the lymphatic system is not, at least in certain cases, the exclusive route that foreign substances take to arrive at the venous system.

This new mode of absorption, much more direct than that by the lymphatics, presents the means of easily conceiving the rapidity with which the different deleterious and other matters are absorbed, as well as the rapidity with which they produce their effects upon the system.

But what are the organs that first absorb the poison from the parts where it has been introduced? Are they the radicles of the veins, or are they rather the capillary lymphatics, which, having immediate anastomosis with the sanguiferous capillaries or exhalents, would immediately diffuse the poison through the venous system?

The experiments which I have just related, joined to those which have been made on the same subject, appear to me totally insufficient to decide either of these questions; only, it ought to be remarked, that our experiments are strongly in favour of a direct absorption by the veins.

But it is a fact, rendered evident by the preceeding experiments, and upon which it is necessary to pause a moment, that the venous blood becomes charged with the poison, and that by the intervention or means of this blood, the poison produces its deleterious action upon the system. In fact, if in the experiments where I had separated the thigh from the trunk, we suspend the course of the venous blood, by compressing between two fingers the crural vein, we lessen, and even totally suspend, the production of the effects. The blood of an animal, in which the signs of action of the *upas* is developed, contains then, some portion of poisonous matter; indeed, it may be said to be really poisoned. It were curious and interesting to know, if this blood carried into the circulatory system of a healthy animal, would produce effects similar to those it had upon the animal itself. At first sight, we are led to believe that this is extremely probable, even that it is certain. The following experiments will shew with what care we ought, in physiology, to distinguish that which is probable from that which is proved by experiment.

We passed the arterial blood of an animal, in which the tetanus caused by the upas was manifest, into the jugular vein of a healthy animal; the transfusion lasted nearly twenty minutes, so that the healthy animal received a very considerable quantity of poisoned blood, which at the first moment of the experiment, was of a red and vermillion colour, and which afterwards became violet and black, when the upas had produced asphyxia. There did not, however, appear any irritation of the spinal marrow, and the animal only experienced what happens in common transfusions, made with care. I mean, that it had, for some hours a very marked acceleration of the inspiratory and expiratory motions, as well as a very abundant pulmonary exhalation. Frequently repeated, this experiment has always produced the same result.

We were now certain, that the arterial blood of animals poisoned by the *upas tieute*, the *nux vomica*, or the *bean* of *St. Ignace*, was not susceptible of producing similar effects on other animals; it would not be, perhaps, the same with the venous blood. It may be presumed, that the respiratory action changed the nature of the poisonous substance; and this alteration might, to a certain degree, give the reason, why the transfusion of the arterial blood of animals poisoned by the strychnos, is not followed with bad effects.

This circumstance did not take place with the venous blood which returns from the part where the poison has been introduced. After the experiments reported in the memoir upon the upas, and in this, it is impossible to doubt that this blood does not transport the poison to the lungs. It is very probable that introduced into the circulatory system of another animal, it would produce effects similar to those which it caused upon the animal on which the inoculation of the poison was made.

A small piece of wood covered with two grains of *upas tieute*, was stuck into the thick part of the left side of the snout of a dog. Three minutes after this introduction, we passed into the venous system of another dog, the blood of the jugular vein of the side where the introduction of the poison had been made. The transfusion commenced about one minute before the first signs of the upas; it did not cease till the death of the animal who experienced it. No appearance of irritation of the spinal marrow was perceived in the animal who received so great a quantity of poisoned blood.

Although these experiments were repeated several times, with variations in the mode of introducing the poison, we never could perceive in the healthy animals, who had suffered in the transfusions of poisoned blood, any thing which resembled the effects of the strychnos.

Results so positive, appear to us of a nature to warrant the conclusion, that the venous blood of animals poisoned by the *upas*, the *nux vomica*, and the *bean* of *St. Ignace*, is no more capable than the arterial blood, of producing upon another animal the effects which it will cause upon the animal from which it was taken.

If there still remained any doubts, they would be removed by the following experiment, which was repeated several times.

As in the experiments above related, we separated from the body the thigh of an animal, insolated as before the crural artery and vein; we introduced the poison into the separated foot, and transfused the blood of the crural vein into the jugular vein of a sound animal. The passage of the blood from one animal to the other, lasted more than ten minutes, a time more than sufficient for the production of the effects of the upas. But no sign of the action of this poison was perceived, either in the one or the other animal. The one preserved perfect health, the other died in a few days, in consequence of the amputation of the thigh, and from the loss of blood which was transfused.

It must not, however, be thought, that, in this experiment, the transfused blood, by some particular cause, had no deleterious properties, for the following experiment proves the contrary.

As in the preceeding experiment, I separated the thigh from the body; three minutes after introducing the poison into the foot, I passed the blood of the crural vein into the jugular vein of another animal; the transfusion was prolonged five minutes without producing any effects. I then stopped it, and disposed things in such a manner, that the blood of the crural vein should return to the animal to which it belonged. Almost instantly this animal exhibited evident signs of the action of strychnos on the spinal marrow.

From the different experiments reported in this memoir, we must, I think, conclude,

1st. That the lymphatic vessels are not always the route followed by foreign matters, to arrive at the sanguiferous system.

2nd. That the blood of animals, upon which the bitter strychnos have produced their deleterious effect, cannot produce any fatal effects upon other animals.

As to the explanation of this singular phenomenon, it would be, I think, premature to give it at present. In physiological science, we ought to be sparing of conjectures, and prodigal of facts.

MAGENDIE, D. M. P.

Note.—This memoir has received the approbation of the Institute.

Dr. Hall's Treatise on the Acetite of Lead.

Dr. Potter—Sir,

AT your request I send you some observations on the tonic powers of the Acetite of Lead, when exhibited internally.

In intermittents of obstinate character, and more particularly in those of protracted duration, I have used the Acet. Plumb. with almost uniform success; and in those cases where an uncommon delicacy of stomach has caused the bark to be rejected, this has produced no unpleasant effect, but has immediately arrested the progress of the disease. In numerous cases a few doses of the above preparation have been effectual when blisters and a variety of tonics had been unsuccessfully used. The dose in which I have generally given it, is to an adult about three grains, and less according to the age of the patient. In the intermission I have commonly directed it to be given and repeated every second or third hour, according to the danger to be apprehended from the return of a paroxysm. In many cases where the remissions were so imperfect as to render the use of the bark doubtful, the Acet. Plumb. was administered with success. It produced no dryness of skin, head-ache, or such symptoms as usually follow from exhibiting the cortex in a febrile state of the system.

In the treatment of the intermittent and remittent fevers of children I have found the above preparation of lead of peculiar utility.*

Its taste and quantity do not produce that aversion which is manifested by children to large and repeated doses of Chincona. I would by no means wish to recommend the article above spoken of in the treatment of intermittent fevers to the exclusion of those which have generally been in use among us, but as a powerful auxiliary to bark and tonics in general. In my own case during the autumn of 1810, from a peculiar irritability of stomach, the bark produced a distressing emesis: the gentian, columbo, and other medicines were equally injurious and the cerus. acet. completed the cure.

---

* My friend and associate, Dr. G. E. Mitchell, informs me that he has also used this medicine with much success in the intermittents and remittents of children.

In those profuse and debilitating nocturnal perspirations which often succeed intermittents, no article of the Materia Medica has under my direction afforded such certain and immediate relief as the medicine of which we have been speaking. In those cases I generally direct it to be taken a short time previous to the hour of rest. From the success which has attended the use of it in the night sweats of intermittents, I have been led to exhibit it with a view to check the sweats of hectic fever. Although it did not as in the former cases so fully answer the intention, yet it generally afforded much relief, by checking in a greater or less degree those profuse and debilitating evacuations.

In no case which has fallen under my observation has the internal use of the cerus. acet. been productive of consequences in the least degree injurious to the constitution of the patient. Yet I would not recommend its exhibition in any case for a length of time, knowing that effects of an unpleasant nature *might* be the result. Such would also be the results from a protracted use of many other articles of the Materia Medica, which are used by all without hesitation. This does not furnish an argument for their rejection, but for the more cautious direction of them by the physician.

RICHARD W. HALL.

Baltimore, 15th March, 1812.

From the Edinburgh Review, for November, 1810.

A letter on the differences in the structure of Calculi, which arise from their being formed in different parts of the Urinary passages; and on the effects that are produced on them by the internal use of solvent medicines, from Mr. William Brande, to Everard Home, esq. F. R. S.—[From the Philosophical Transactions for 1808, Part II.]

Some observations on Mr. Brande's paper on Calculi. By Everard Home, esq. F. R. S.—(From the same volume.)

An account of a Calculus from the human bladder, of uncommon magnitude. By Sir James Earle, F. R. S. [From the Philos. Trans. for 1809, Part II.]

Observations on the effects of Magnesia in preventing an increased formation of Uric Acid; with some remarks on the composition of the Urine....Communicated by Mr. William T. Brande, F. R. S. to the Society for the improvement of Animal Chymistry, and by them to the Royal Society.......(From the Phil. Trans. for 1810, Part I.)

On Cystic Oxyde, a new species of Urinary Calculus. By William Hyde Wollaston, M. D. Sec. R. S.......(From the same work, Part II.

THESE curious and valuable papers relate to one of the most excruciating torments with which Divine Providence has, for wise purposes, decreed that the lot of man should be mingled, during his existence in this world. Indeed, were it not that custom teaches us to consider mental afflictions as, in the general, more severe than any bodily pains, (and, no doubt, to some natures, they are beyond all comparison, more unbearable,) we should be disposed to rank the dreadful complaint which we are about to treat of, as the worst of human maladies. To investigate the nature of the Stone, for the purpose of discovering solvents which might remove it, has accordingly been long considered as one of the noblest problems of practical chymistry, and among the best services which that science could render to the healing art. It may be remembered, that to this investigation we owe the brilliant discoveries of Dr. Black, which, by disclosing the nature of fixed air, and of the alkaline earths, paved the way for all the modern improvements in chymical knowledge. Those great acquirements in speculative science were the compensations which he obtained for his disappointment in a search far more important and interesting to humanity: as the navigators of the fifteenth and sixteenth centuries discovered the new world, and explored its strange recesses, while occupied in the pursuit of an earthly paradise, or a fountain of health and youth. Even the legislature of this country took some part in those inquiries, and gave rewards to the discoverers of solvents. The most noted instance is that of Stephenson; and here the money was granted upon the faith of a cure *said* to be performed, the body not having been examined after death. We need scarcely add, that medicine thus patronized, and, as it were, privileged, has long since been completely exploded; and though, in its failure, the parliament resembled the celebrated philosopher whose name we have mentioned, we do not remember that its investigations were like his, rewarded by any other discoveries.

The hopelessness of the search for *solvents*, has lately turned the attention of the chymists and physiologists to another problem; and they have been occupied in ascertaining the process of calculous formations, with the view of administering such *preventive* medicines as may indispose the system to produce those concretions, or check their growth, without altering the tone of the constitution. The papers now before us throw considerable light on this important subject; and contain also a good deal of information, tending to elucidate the fallacies of the older doctrines respecting solvents. Indeed, did we not know the danger of trusting to medical reports of cures, and did we not recollect how regularly a series of cases, all quite apt and most nicely fitting the doctrines, form a part of every modern treatise on medical subjects, we should be tempted to consider a great part of the question solved with regard to the stone, and should confidently look to the extirpation of that worst of maladies. Even moderated by such unpleasant recollections, as we own our hopes to be, we cannot look to the eminent names prefixed to these tracts without great respect; and we feel at least so much inclined to listen, that we shall make no farther apology for laying before our readers an account of what the papers have added to the stock of our knowledge on this subject.

In order to convey some idea of the greatest degree in which this disease has ever yet, we believe, been known to afflict its wretched victims, we shall begin with noticing the case narrated in Sir J. Earle's paper.

Sir Walter Ogilvie, a Scotch Baronet, received, at the age of twenty-three, a severe blow on the back, from the boom of a vessel; and the pelvis and lower extremities were in consequence paralyzed. He remained in bed for fourteen months, in an horizontal position; and, during the first two months, his urine was of necessity drawn off by a catheter. After this, he had somewhat recovered the use of his limbs, and could walk or ride, though with difficulty: his general state of health, too, was weak and precarious. About twenty years after the accident, he was afflicted with symptoms of stone; and upon examination, a calculus of considerable size was found to have been formed in the bladder. An operation was now recommended, but, unfortunately, delayed from time to time, although the patient's health constantly declined, and the irritations and pains in the bladder greatly increased. In this melancholy state he continued for eight years longer, when he became unable to make water in an upright position: he was obliged to place himself in an inverted posture, that the upper part of the bladder might become the lower; and as, even by this means, very small quantities of urine only could be voided, the irritation of endeavoring to make it became almost perpetual, and he was obliged to perform the above mentioned operation every ten minutes, with scarcely any relief. The principal remedies which he tried were *aqua* *calcis* and *uva ursi.* He used opium also to alleviate the pain; and this, adding to a naturally costive habit, rendered aperient medicines frequently necessary, which, again, must have increased the irritations of the disease. At the age of fifty-three, thirty years after the accident, the spasms became so constant, and so violent, that he resolved to have the operation performed; and the paralysis of the lower parts being deemed no obstacle to it, he was conveyed by sea to London, where Sir James Earle and Mr. Cline consulted upon his truly lamentable case, about the end of July, 1808.

Upon examining the lower part of the belly, a tumor was perceived, so large, that sir James Earle doubted its being altogether caused by the stone; but, on sounding, the instrument would not enter the bladder; and further examination showed, that the whole cavity was nearly filled with calculus. It was clear, therefore, that this could only be extracted, if it should prove soft enough to admit of being broken, and thus brought away piecemeal; and it was deemed impossible to attempt the operation above the *os pubis.* The ordinary method was therefore resorted to; and after some day's consideration, sir Walter Ogilvie, well aware of the difficulties of the case, and of its great uncertainty, yet justly weary of the long life of torment which he had led, was resolved to have the operation performed, for the chance which it afforded of bringing a relief, only otherwise to be attained by death. It was accordingly performed by Mr. Cline, on the 11th of August.

He was placed in the usual position, and the ordinary ligatures were applied; but the paralysis of the lower limbs rendered it unnecessary to confine them, and they were left at liberty. It was found impossible to pass the staff, or any kind of forceps, into the bladder; but on pressing hard with the finger, part of the stone felt soft, and gave way. This making room for the forceps and scoop, some parts of the concretion were brought away; and, in this manner, about a large teacup-full was extracted. The great mass, however, continued hard and immovable: it could neither be broken nor shaken from its position; and after trying every instrument, of all powers, the operator was compelled to desist, leaving it (with the exception of the soft matter brought out) in its former state. The patient bore this long process with great fortitude: happily, the paralysis of his lower regions may be supposed to have somewhat diminished the sensibility of the parts. He was, however, completely exhausted, from weakness and fatigue.

Although no hemorrhage, nor any fatal symptom, appeared to result from the operation, it had afforded no sort of relief; and the spasms continued as before. They soon increased in frequency and effect, as he became exhausted, and unable to stand them. He seems to have had a gleam of hope on the morrow of the operation; for he frequently inquired when he might be well enough to have the remainder extracted. But he gradually sunk under the repeated and violent irritations of the spasms. On the eighth day, his pulse became smaller and quicker, and he could scarcely be prevailed on to take any sustenance: on the ninth, he was more impatient, feverish and restless; and on the tenth day, "he desired not to be teased to take any thing more; when covering himself completely with the bed-clothes, he quickly resigned a most singularly miserable existence."

On dissection, the bladder was found to be *quite filled* with a huge calculus, composed of the triple phosphate, which we shall afterwards have occasion to speak of; and of such an unusual proportion of animal matter, that it speedily showed a strong tendency to putridity, and even engendered maggots. It weighed forty-four ounces, or three pounds four ounces, apothecary's weight. Its form was that of an irregular ellipsoid; the larger circumference being sixteen inches, and the smaller fourteen. It had filled the bladder, and taken its shape, but apparently without distending it. The ureters had become enlarged, so as to contain the urine, and act as small bladders, while the large bladder only permitted that fluid to paſs off by means of ſmall furrows which it made in trickling along the ſurface of the ſtone, between the ſtone and the bladder. There appears no reaſon to doubt, that, in a ſhort time, even this painful and inſufficient operation would have become impracticable, by the further enlargement of the ſtone ſtopping up the mouths of the ureters; in which event, death muſt have enſued from ſuppreſsion.

Such, in its very worſt ſtate no doubt, is the dreadful malady which forms the ſubject of the chymical papers now before us. To theſe we muſt now direct our attention; and we can promiſe the reader ſufficient reward for his trouble, if he ſhall be pleaſed to follow us through the analyſis of their contents.

Mr. Brande's valuable paper, which ſtands firſt in the prefixed liſt, is founded on an examination of the collection of calculi contained in the Hunterian muſeum. Beſides the known richneſs of that collection, moſt of the ſpecimens have hiſtories of the caſe annexed to them, which are obviouſly of the greateſt uſe in the inveſtigation of the ſubject. Mr. Brande arranges calculi into four claſſes: firſt, thoſe which are formed in the kidneys, and voided without undergoing any change in the paſſage; ſecond, thoſe which are retained in the kidneys; third, thoſe which are found in the bladder: and, fourth, thoſe which are found in the urethra. On each of theſe ſpecies, our author has made a number of experiments; and the reſults exhibit a ſufficiently diſtinct account of their chymical analyſis. The firſt claſs, or calculi of the kidneys, conſits almoſt always of uric acid (the Lithic acid of Scheele) and animal matter; the animal matter is found in various proportions, from one-eighteenth to one-third of the whole compound. Sometimes the acid and the matter are not formed into a ſolid calculus, but paſs off mixed with phoſphates. The author, however, is diſpoſed to think that the phoſphates are not elaborated in the kidney, but mingle with the calculous matter in its paſſage. In ſome few inſtances, the calculus contains oxalate of lime; but this is very rare. The ſize of theſe calculi varies, and one has been deſcribed by Dr. Heberden, so large as to weigh twenty-eight grains; but it may have been detained on its passage, and so augmented in bulk. The largest which Mr. Brande examined only weighed seventeen grains. All the calculi of this class are certainly soluble in pure potash; but it is needless to add, that the caustic qualities of this substance render its application in the living subject impossible.

The second class is formed by the detention of a calculus in the kidney, while either the secretion of uric acid enlarges its bulk, sometimes so as to form a complete cast of the pelvis of the kidney; or the stream of urine to which it is exposed deposits the phosphates upon the original nucleus of uric acid and animal matter. In the one case, we have an enlarged calculus of the first class, in the other a calculus of the third class, only detained in the kidney.

When the uric calculus passes into the bladder, and is there detained; or when a calculus of oxalate of lime comes down from the kidney; or when portions of sand or animal mucus are deposited in the bladder; or when any extraneous substance has found its way thither and is detained—in all these cases, that is, on all these substances as nuclei, there are formed calculi (commonly termed stones in the bladder) with greater or less rapidity, according to the tendencies of the constitution. They may be ranged into three subdivisions: those composed chiefly of the uric acid; those chiefly composed of phosphorates (the ammoniàco-magnesian phosphate, or phosphate of lime, or a mixture of the two;) and those containing oxalate of lime, which are commonly termed mulberry calculi. Messieurs Fourcroy and Vauquelin, and Dr. Pearson, having found the greater number of calculi to be composed of uric acid (that is to say, belonging to the first of these subdivisions,) our author was surprised to find that, in the Hunterian collection, this proportion did not hold; for, of a hundred and fifty which he examined, only 16 were composed of uric acid wholly (beside the animal matter:) 45 of that acid with a small portion of phosphates: and 5 of the acid with phosphates and nuclei of oxalate of lime; while 66 contained phosphates with a small admixture of uric acid; 12 the phosphates entirely; and 6 chiefly oxalate of lime.—Observing that a greater loss was sustained in obtaining uric acid from calculi in the bladder, than from calculi in the kidney, Mr. Brande was led to conclude that the former contained urea. With a view to ascertain this point, he made some experiments, and found that an uric calculus, weighing 60 grains, contained 5.2 of urea and muriate of ammonia, 6 of ammoniaco-magnesian phosphate, and 48.8 of uric acid: but no urate of ammonia could be detected: from whence, and from other experiments, our author infers, that this substance, when supposed to exist in calculi, owes its origin to the development of ammonia, and its recomposition with uric acid in the course of the processes of decomposition. The mulberry calculi (which are rare) contain, in general, a considerable admixture of phosphates and uric acid with the oxalate of lime. The very large calculi often contain an agglomeration of different species of calculi. Mr. Brande examined one, of twenty-three ounces and twenty-six grains, which had a nucleus of uric acid, surrounded with a considerable quantity of very pure ammoniaco-magnesian phosphate: the mass of the stone consisted of mulberry calculus. Another, weighing 15 1-2 ounces, had a nucleus of uric calculus, round which was a mass of ammoniaco-magnesian phosphate, interesected with láminæ of üric acid. It is rare to find calculi composed of four distinct substances. Our author saw one which had four separate layers, or strata, of uric acid, oxalate of lime, ammoniaco-magnesian phosphate, or phosphate of lime. He examined four calculi, the nuclei of which were extraneous substances, viz. a garden pea, a needle, a hazel nut, and part of a bougie. Besides the animal matter, the first calculi contained phosphates only; the second, phosphates and oxalate; and the other two were also destitute of uric acid.

The fourth class of calculi, those found in the urethra, offered no peculiarities to the observer. Indeed, the classification of Mr. Brande does not refer so much to any distinct properties of the substances arranged, as to their position and accidents; their *habitat*, as the botanists express it; for their properties seem to depend merely upon their exposure to the urine in the different stages of its elaboration; nor is there any manifest variety of circumstances affecting their formation, except in so far as they are found in streams of that fluid in its nascent or in its perfect state. The calculi in the urethra accordingly, are only small calculi which, passing from the bladder, have been detained and lodged in the membranous part of that canal, and, there, have been exposed to the same process of slow accretion, from the passage of urine, which they would otherwise have undergone, from lying in the bladder exposed to the action of the same fluid. One of the calculi of this description, which Mr. Brande examined, was of a singular appearance. It was a perfect sphere, of about half an inch in diameter, coated with small regular crystals of the ammoniaco-magnesian phosphate in its purest state.

Our author closed his experiments with examining the calculi of different animals. In the horse, sheep, rhinoceros and rabbit, the calculi consisted of the phosphates in part, with animal matter and carbonate of lime. In the dog, no carbonate of lime was found; and, in the hog and ox, no phosphates; the calculus being wholly composed of carbonate of lime and animal matter. No uric acid nor oxalate seems to have been detected in any of the specimens. Some of them were of a large size: a calculus from an old dog weighed sixteen ounces.

From this indication, it is very evident that one conclusion may safely be drawn: that, in most instances, the stone is connected with the formation of uric acid. Sometimes that acid forms calculi in the kidneys, which after creating sufficient torment in that region, pass into the bladder, and become the nuclei of still larger, more painful, and more fatal accretions of other substances; while, in many instances, the same acid continues to augment the mass, either alone or combined with other substances. To prevent its formation, then, becomes a most important object; and both Mr. Brande and Mr. Home propose, for this purpose, the exhibition of some alkaline earth, which may neutralize and carry it off. But, before adverting to this preventive remedy, we may notice the additional proofs adduced, in the papers now under review, to evince the hopelessness of any attempts to solve the stones already formed, at least in the present state of chymical science:

In arguing this point, Mr. Brande (or, as Mr. Home always terms him, Mr. W. Brande) proceeds more by principle; his patron Mr. Home comes forward to his protection with cases. Mr. Brande clearly shows, that an alkali administered to a calculous patient stands no chance of reaching the uric concretion in a caustic state, for the urine contains both phosphoric and carbonic acid uncombined. But experiment clearly shows, that neither carbonates nor subcarbonates exert any sensible action on uric acid: in other words (as we apprehend) the affinity of the uric acid for alkalies is weaker than the affinity of crabonic acid for the same bodies: therefore, alkaline liquors cannot act as solvents of the uric calculi. On the other hand, if acids be administered with a view of attacking the phosphates, the formation of the uric acid is greatly favoured. This, Mr. Brande's experiments have also taught him. And as for the injection of solvents through the urethra into the bladder, he justly observes, that although we were not ignorant (as we are) of the kind of calculus in each case, the frequent introduction of instruments into the bladder, and the long continuance of the operation, would form insuperable obstacles to this mode of treatment, which, though recommended by great names, has always been speedily relinquished when tried.

Now come Mr. Home's cases: which plainly demonstrate, that where the use of alkaline medicines was supposed to have relieved the patient by dissolving the calculus, on examination no such effect was found to be produced. In two cases, where the violence of the complaint appeared to have subsided, the dissolution was taken for granted, and ascribed to alkaline medicines; but, on dissection, the calculi were found in great size, only imbedded in cysts, from the enlargement of the prostrate gland, which often takes place late in life. In some instances, again, the increase of calculous concretions was found to proceed rapidly, while the patients were going on with courses of alkaline medicines. One person took these remedies for four or five years, and at his death the bladder was found nearly filled with light, spongy calculi of different sizes, no less than three hundred and fifty in number. Another, who had taken soda, both mild and caustic, for some months, and then submitted to the operation on the symptoms increasing, was found to have a calculus, which was surrounded with a coat of triple phosphate, one tenth of an inch thick, the rest being a mixture of uric acid and phosphates; from whence it is fair to infer, that the alkali increased the formation of triple phosphate, though it checked the production of uric acid. In a third case, of a very virulent nature, the exhibition of alkalies did not even prevent the formation of uric concretions. Mr. Brande's experiments however, in 1808, having rendered it highly probable that, in the great majority of cases, alkaline medicines would operate powerfully in checking the growth of uric calculi, by neutralizing the uric acid before it could form a concretion, this inquiry has lately been resumed by that industrious and skilful observer, with the aid and advice of Mr. Home; and the result of their joint labours is given in the fourth of the papers examined at the head of this article.

Upon consultation with Mr. Hatchett, they were led to fix upon magnesia, in preference to the other alkaline substances; and the event appears to have justified this very natural and judicious choice. Some preliminary trials showed, that the mild, safe, and easily prepared earth in question, diminished the quantity of uric acid in urine, which contained a larger proportion of it than any of the alkalies, however copiously administered. But this point is of such primary importance, that we must notice the four cases in which a further examination of the matter was prosecuted.

The first was that of a gentleman sixty years old, who, apparently from the habit of drinking acid liquor, had acquired an uncommon tendency to secrete uric acid, and had his urine constantly mixed with that substance, in the form of red sand or crystals. He took, first, the subcarbonate of soda, and then of potash, without any amendment from the former, and with but a slight relief from the latter. His urine being previously examined, he was ordered to take fifteen grains of magnesia three times a day, and in a week the uric concretions diminished sensibly: in three weeks, they were only observed occasionally. The same medicine has been continued for eight months, and no calculi have been voided; nor has there been any material deposit in the urine. He has also been cured of heartburn, and other stomach complaints. Another gentleman, of about forty years of age, who suffered greatly from a similar complaint, and tried the alkalies in vain, has been wholly cured by a six weeks' course of magnesia, in the quantity 'of twenty grains every night and morning,' (we wish Mr. Brande would say more distinctly whether he means *twenty* grains a day, or *forty*,) without any change of regimen whatever. The third case, is that of a gentleman forty-three years old, who has, for about a year, succeeded in driving away repeated attacks of the uric acid, by taking magnesia for a few weeks at a time, when the fit comes on. The fourth case is that of a more confirmed calculous tendency, which has nevertheless yielded, in a great degree, to the free use of magnesia; with this addition, that since the patient began it, he has been free from gout, contrary to his former habits.

The different qualities of magnesia and the alkalies thus observed, led our author to make some experiments on their respective effects upon *healthy* urine. Subcarbonate of potash and soda occasioned a copious precipitation of the phosphates in the urine, during the first two hours after these salts were taken into the stomach; and, after that, no further effect was produced. The same alkalies, saturated with carbonic acid, threw down the phosphates less copiously and rapidly. Magnesia, administered in the same circumstances, produced a much smaller and slower precipitation. Instead of this effect reaching its maximum in a quarter of an hour, as it did in the case of the alkalies, it did not reach that point in less than six hours; and on this circumstance our author concludes, that its value in calculous disorders chiefly depends. Lime, either in the caustic or mild form, produces a very slight effect; and its nauseous taste, as well as difficulty of administering it in sufficient quantities, manifestly preclude its use in this disease. In one case, carbonic acid, exhibited in aerated water, was found to keep the phosphates (which the urine was greatly charged with) in a state of solution; and, when left off, those salts were again voided in their solid shape.

Thus far, then, we think, it may safely be hoped, at least with as little risk of disappointment as usually attends our speculations in medical science, that some light has been thrown on the method of treating a disease, of all others which prey upon the body, the most inimical to the happiness and comfort of man. Where the object is so valuable, it is natural for us to feel uncommonly anxious and distrustful, as well as more than usually eager in our wishes for its accomplishment. Instead, therefore, of being satisfied with what Mr. Brande and Mr. Home have already effected, we would expect them to persevere in their experimental inquiries: multiplying the number of their observations on real cases: and, as it were, leaving no stone unturned to attain the complete mastery of this great problem. Much will be done for medicine, if they should go no further than perfecting the preventive application of magnesia to the uric calculus. A great number of calculous cases, of the worst description, will be prevented: those, to wit, in which the stone in the bladder is formed on a uric nucleus. Many others of the same class will be greatly relieved. Those, in which composite calculi consist partially of uric acid; and all that class of complaints with which the region of the kidneys is afflicted, by the uric concretions formed there, will be entirely removed. Should such a step in the healing art be made, we need scarcely despair of living to see the phosphates themselves attacked by it, and yielding to some equally simple and safe remedy.

While we are, however, indulging in these visions, and anticipating the final eradication of all calculous disorders, we unluckily cast our eye on the last of the papers which are now before us, and find, that Dr. Woollaston has been discovering a new species of urinary calculus, at the very moment that his learned brethren were occupied in extirpating the already sufficiently numerous specimens of the old catalogue. This new species is extremely rare, our author having only met with it twice. It resembles the triple phosphate more than any other kind; but is more compact, and consists, not of laminæ, but of a confused mass of chrystals, having a yellowish lustre and semitransparency; but it is formed of a peculiar substance. Dr. Woolaston made as many experiments on this substance, as the limited quantity of it in his possession would allow. When burnt, it gives a smell quite peculiar to itself. When distilled, it gives a fetid carbonate of ammonia, and an animal oil, leaving a spongy coal. It is readily dissolved by all the pure alkalies and by lime water: it is even solved by the carbonates of potash and soda. The acids dissolve it also, except the citric, tartaric, and acetic. Neither water, alcohol, nor saturated carbonate of ammonia, dissolve this singular substance. From its disposition to unite with both acids and alkalies, Dr. Woollaston suspects it to be an oxide; which is confirmed by its forming carbonic acid in distillation; that is to say, if we take it for granted, as he seems to do, that this acid does not exist ready formed in the mass. If it does contain oxygen, our author admits, it must be in a quantity insufficient to give it acid properties; for it produces no effect on the color of litmus paper. On the supposition of its being an oxide, and to distinguish it from other bodies of that class, Dr. Woollaston proposes to name it the *cystic oxide*, on account of its having hitherto been only found in the bladder.

This excellent chymist concludes his paper with some curious observations on the connexion between the production of uric acid in birds, and their food. The white matter contained in their urine, and voided along with their dung, was found by M. Vauquelin to consist principally of that acid; and our author examined with some care in what manner its quantity was affected by the diet of those animals. In the dung of a goose fed on grass, it only formed 1·200 part; in that of a pheasant fed on barley, it amounted to 1·14; in that of a hen which fed on insects as well as vegetable food, in a barn-yard, it was much more abundant, and mixed with lime. The solid part of the dung of a hawk fed wholly on flesh, was chiefly uric acid; and the evacuation of a gannet, feeding solely on fish, consisted altogether of urine; for the only solid parts were uric acid. "It seems, (our author concludes) deserving of inquiry, what changes might be produced in the urine of any one animal by such alterations of its diet as its constitution would permit: for, as far as any inference can be drawn from these varieties which naturally occur, it would apear that persons subject to calculi consisting of uric acid, as well as gouty persons in whom there is always a redundance of the same matter, have much reason to prefer vegetable diet; but that the preference usually given to fish above other kinds of animal food, is probably erroneous."

The mention of Dr. Woolaston's paper naturally leads us to reflect on the important services which this truly philosophical inquirer has formerly rendered to the branch of science now under consideration. It was his important discovery of the nature of gouty concretions, which first ascertained the analogy long suspected between gout and stone; and, besides giving the first analysis of those concretions which had ever been experimentally attempted, he also first (with the able assistance of Mr. Tennant, a name far too well known to need the humble tribute of our admiration) investigated the composition of the calculi, consisting of neutral salts. The discovery of Scheele, perhaps his most important discovery, brought to light the nature of uric calculi; and, at the same time, made us acquainted with a new acid. The existence of this acid was suspected in gouty concretions; and it was generally imagined that they consisted of the acid alone, or combined with animal matter only. Dr. Woollaston showed by experiment that they are composed of uric acid and soda. Until he carried his inquiries into the other classes of calculi, it was only by conjecture or vague analogies that their constitution was known; but to him we owe as perfect an acquaintance with their component parts, as Scheele had left us of the uric calculus. Having thus completed our analysis of the calculi formerly known, he has, in his present communication, brought to light both a new calculus, and a body. His former paper was published in the Philosophical Transactions for 1797; and the one now before us forms an important appendix to it. It is not within our province to take any further notice of the first tract; but we could not conclude this article without recalling it to the recollection of the scientific reader, and reminding him of his great obligations to Dr. Woollaston.*

---

* See also a valuable collection of historical notices, as well as analytical researches, on urinary concretions, in Dr. George Pearson's paper, Phil. Trans. 1798, Part I

A review of Dr. Mease's Case of Varicose Aneurism, by John B. Davidge, M. D. Professor of the Institutes or Principles of Physick in the Medical College of Maryland.

MEDICAL cases, collected with assiduity, examined with intelligence, and promulgated with ingenuousness, are ample lessons of instruction, but when they are permitted to be vehicles of error, or channels affording facilities to undijested, hasty speculations, they call irresistibly on our attention, and rouse our vigilance ; they should be scrutinized with rigor, yet with justice, and admitted with the extremest caution ; it becomes a duty to control their influence, and repress their extravagance. Were it so that they would fall into the hands of men of science and observation only, we should give ourselves but little concern about them, but as they are intended to convey, and do convey, doctrines and principles, to minds incompetent to determine on their nature, it would amount to culpable dereliction of the duty we owe both the public and our profession to let them pass unnoticed. In an especial manner are we bound to subject such cases, as are here alluded to, to strict and critical analysis, where they come forth clothed with the authority of character, and enforced by the recommendation of that celebrity which already and deservedly possesses the public confidence.

Had the above case, to be found in the sixth number of the Philosophical Journal and Review of New-York, from the pen of Dr. Mease, been given to the world unaccompanied by the *preface* and *conclusion* by Dr. Dorsey, it would have excited little interest and awakened less curiosity. There is so much of common character about it; so much of what every professional man, at all in the habit of using the lancet, must frequently meet with, that it would have called forth few other sentiments than those of gratitude for the benevolent intentions of the respectable author, and, perhaps, surprise at the minuteness of a detail without the force of conviction or ornament of variety.

Upon reading the prelude, on which the editor of the Journal in a foot-note, makes a judicious remark, we acknowledge we anticipated something curious and valuable; such as, if it did not illustrate what the writer wished to establish, would at least have afforded entertainment to the practitioner and information to the pupil; but, in this our expectation, we were disappointed. The first point, in the detail of the case, deserving of remark, is "The blood at first appeared *very dark*, for I had been in high health."

That the blood when it first flows from a vein, recently opened, should be *very dark*, may be attributed to almost any circumstance rather than to *high health*. We presume the author means by very dark, *unusually* dark.

The blood may be very (unusually) dark from any disease which may so affect the lungs in their function as to prevent the blood from being properly oxigenated; or from the air being cut off from the lungs by submersion, or a cord round the neck. Contrary to what is the import and meaning of the writer's language, the blood is never very, or in any eminent degree, dark, exclusively from high health. It is true it may be very dark when the arm has been bound up any considerable time before the vein be opened, although the persons have been in high health. And we are not a little surprised that a gentleman of the professional respectability of Dr. Mease, should have permitted an idea so unphilosophic to have escaped from his pen.

"In about ten or twelve seconds it (the blood) changed to a florid complexion, and was discharged in jets."

From this the writer wishes it to be inferred that the former quantity, the very dark, was from the vein, and the latter from the artery. And to fortify this suggestion still more, he is careful to mention that it was discharged in jets.

If a ligature be passed round the arm for a short time and then the vein be opened, the blood first discharged will be dark in proportion to the time the ligature was on previous to the opening of the vein; if it be permitted to flow for a minute or two it becomes of a colour more or less florid; and if the vein opened be the basilick, running immediately over the artery, the blood will be discharged in jets. Nor will those circumstances occur casually; they are almost always uniform.

The blood is dark because it is detained in the veins of the arm, as it were out of the general circulation; it becomes of a florid complexion because there are fresh supplies round from the arteries; and it is discharged in jets because the great artery in the fold of the arm is strongly beating under the lower wall of the basilick vein. There is nothing in all this which can *decisively* point out the artery to be wounded, or should have led a practical man to have suspected such an accident.

"I felt the pulsation of the blood, while flowing from the artery into the vein."

Of this, no doubt, the Doctor felt satisfied, but he will excuse us, we hope, when we acknowledge that we can have no satisfactory conception how, or in what way, he could determine whether the pulsation he felt were of the blood flowing from the artery into the vein, or from the blood flowing along the channel of the artery, beating against the blood effused from the vein. We may repeat, that to determine the pulsation felt to have been from the blood flowing from the artery into the vein, required more nicety and accuracy of touch, than in our estimation could have been boasted by any man, how well soever trained his sensation might have been; and we are still disposed to propound the question: Is the pulsation of the blood while flowing from the artery into the vein obviously different from the pulsation of the blood while flowing in the tube of the artery? and, If it be not indicated by some marked variety, might not the Doctor have been imposed on?

"The *pulsation* of the *blood*, is an unusual expression. We understand the *pulsation* of an *artery*.

"And perceived the thrilling noise from the same cause whenever I put my head under the bed-clothes."

How strongly soever the Doctor was persuaded that he perceived a thrilling noise whenever he put his head under the bed-clothes, we must be indulged in the opinion that there was more of *imagination* than *reality* in the perception. Fancy is sometimes extremely unruly, and at no time more so, than when a man had become persuaded that the main artery of the arm was wounded. Particularly when the subject of this supposed injury is a physician, well acquainted with the consequences of such a wound.

Why could not the thrilling noise be heard except when the Doctor was in bed? Is *darkness* favorable to sound? It is to the exercise of the imagination. Or are vibrations propagated better under the bed-clothes than in the open air of the room? It does not appear that this thrilling noise was heard by either Dr. Physic or Dr. Dorsey.

"I was careful to use my arm as little as possible, for besides the continuance there was a small swelling about the size of a small filbert in the injured part; but about five or six weeks after the accident I imprudently exercised it for half an hour, when the part swelled to the size of a common filbert, became painful and looked red. The pain and redness however soon subsided, but the swelling did not diminish for upwards of two months."

Now we have before us the plain and unadorned fact; the simple description of an ecclymosis compounded with an inflamed and thickened tendon; such as we have all met with so often that we have almost ceased to notice them.

In a genuine varicose aneurism, the blood passes freely and uninterruptedly from the artery into the vein, especially so when a thrilling noise can be heard. If then there be a liberal and ready passage of the blood from the artery into the vein, and afterwards an easy progress on through the vein to the heart, how could an increase of swelling, a redness and pain occur? This pain, this redness, this increase of swelling, argues very strongly, to our understanding, that the case was an ecchymosis combined with inflammation and thickening of the circumjacent substance. Indeed except there be a prompt and ready passage of the blood from the artery into the vein, the vein can never become varicose; and if there be this ready passage of the blood we cannot understand how, at the distance of six weeks after the injury occurred, inflammation could take place; but that it did take place is obvious from the phenomena of redness, pain, and swelling.

"But, says the Doctor, the swelling did not diminish for upwards of two months."

If the case were a genuine case of varicose aneurism, what could cause the *swelling* to diminish at all, until the aperture in the artery were closed? The vein being once varicose, would continue to be so, and, indeed, increase in volume, so long as the blood should have free access to it from the artery.

"Doctor Physick having carefully examined the arm, was perfectly convinced of the existence of varicose aneurism and did not entertain the faintest hope of a spontaneous cure."

The opinions of Dr. Physick are respectable, and we are at all times gratified in paying regard to them; but we are desirous it should be understood that the opinions of that gentleman, like those of every other gentleman of science and observation, must be considered as opinions, *mere opinions*, and can by no means be substituted for facts. And we are the more unwilling, in the present instance, to admit the substitution of *opinions* for *facts*, as we are informed that the able Physician referred to had not the faintest hope of a spontaneous cure. That is, neither his reading or experience had ever furnished him with a spontaneous cure of a varicose aneurism, and conceiving this to be one, he had no ground on which he could bottom his hope.

If neither the fair pages of surgical history, nor the honest experience of any judicious Surgeon, can furnish a solitary case of a spontaneous cure of a varicose aneurism, or indeed any other, without the obliteration of the diameter of the artery wounded, ought there not to be some little hesitation in making up a decisive opinion in the present case? But even granting that the artery was wounded, that it was an unequivocal aneurism, what evidence has Dr. Dorsey, from which he can reasonably or justly conclude that "the artery ever since has been pervious."

"At this time and ever since the cure, the artery has been pervious, and pulsates like that in the other arm at the elbow and wrist."

In this concluding sentence Dr. Dorsey indulges himself in an unreserved expression of a positive opinion on a subject, respecting which he could have had no decisive or positive knowledge. We will grant however to show more plainly the inconsiderateness of the Doctor's conclusion, that the artery was wounded, and that there has been a spontaneous cure. But does it follow as a necessary consequence from a spontaneous cure that the artery must be pervious? There are numerous instances of spontaneous cures of aneurism on record, but in all there was an obliteration of the cavity of the artery. On the contrary there is no case where the wound in the wall of the artery has been closed by accident or by nature, leaving the artery *pervious.*

The lumen of an artery may be obliterated to a certain extent and yet, in consequence of the touch by the finger, being confused by the pulsation of the artery above, and below the point of union or consolidation of the sides of the artery, no satisfactory or unexceptionable deduction can be established.

With regard to the pulsation at the wrist, on which some stress is laid, we hope it is unnecessary for us to suggest, to the learned gentleman, that the perviousness of the main artery at the fold of the arm is by no means necessary. He well knows, as every surgeon must know, that an artery pulsates, when supplied by anastamosing branches, its trunk being tied or cut asunder, as regularly and nearly or quite as forcibly, as when its diameter is free and open throughout.

It appears to be forgotten that there was, and that too for a considerable time a compress on the tumour. What prevented the sides of the artery from being forced into contact by a compress continued for a series of days? And if forced into contact, what prevented a union?

But Dr. Dorsey decisively and unhesitatingly avers that the artery is pervious. And by what sensible means has he ascertained this? we know of but one mean, unequivocal and satisfactory, by which this point can be settled, that is *injection.*

Surely gentlemen must be very solicitous to sustain the notion that wounds in arteries can be cured spontaneously without a closing of their cavites, and that the surgeon need not trouble himself about the ligature or compress which we declare to be *necessary* in all wounded arteries.

We will conclude the present remarks, with hinting to these gentlemen, and those who may fall in with their cases, that, if they do not obliterate the diameters of wounded arteries by compress or ligature, the fate of their patients will remain as sad memorials of the imperfect condition of the surgical art; and we will subjoin that we exceedingly regret, because we highly respect Dr. Dorsey, that, without *positive* proof of any kind, or *uneqvivocal* evidence of any sort, or even *probability*, he has permitted himself to publish the following paragraph: he should have recollected that a sufficient number of lives have already been lost from wounded arteries left to "heal without an obliteration of their cavity."

"In compliance with my promise, I have the pleasure to subjoin the case of anuerism which I consider a *decisive* proof, that *wounded arteries may heal without an obliteration of their cavity.* I fully concur with you in the opinion that such a termination is extremely rare; but those writers who pronounce the thing impossible, are surely mistaken"!!

Medical and Philosophical Intelligence.

Prize Questions for 1812.

THE committee appointed by the corporation of Harvard University, to carry into effect the laudible views of Ward Nicholas Boylston, esq. do hereby engage to deliver to the writers of the best dissertations on each of the following subjects, respectively, a Gold Medal, of the value of thirty-three dollars, or that sum in money, at the option of the successful candidate in each case, viz.

1. On Epilepsy—its varieties, and the mode of treament of each.

2. What substitute can be had for leeches, in order to draw blood gradually, with little or no pain from small surfaces; and which from cheapness and facility of application, in the hand either of the patient or of the medical practitioner, shall be adapted to general use.

3. On the medical uses of carbonic acid gas.

The dissertations must be transmitted (post paid) to Lemuel Hayward, esq. M. D. Boston, on or before the 31st day of December, 1812. Each dissertation must be accompanied with a sealed packet, on the outside of which shall be written some device or sentence, and on the inside, the author's name and place of residence. The same device or sentence must be written on each dissertation as on the packet which is attached to it. Dissertations with the names of the authors affixed to them, will not receive any attention from the committee.

Unsuccessful dissertations shall be returned, if desired, with the sealed packets unopened; or if this be not desired, the sealed packet shall be destroyed by the committee, without breaking the seal.

LEMUEL HAYWARD,
JOHN WARREN,
JOHN BROOKS,
THOMAS WELSH,
AARON DEXTER,
JOSIAH BARTLETT,
WILLIAM SPOONER,
JOHN FLEET,
JAMES JACKSON,

Boston, May 6. 1812.

Mr. George Myers, of this city, has discovered, in the neighbourhood of Baltimore, a perfect specimen of the *Sulphuret of Molybdæna*, the first we believe that has been found native in the United States.

College of Medicine of Maryland.

At a stated meeting of the Regents, on the 6th of January, 1812, George Brown, M. D. having signified by letter his resignation of the office of President, Charles Alexander Warfield, M. D. was unanimously elected.

At a stated meeting of the Regents, on the 6th of April, the following adjustment of professorships was adopted, viz. James Cocke, M. D. Professor of Anatomy; John B. Davidge, M. D. Professor of the Institutes of Physic; Nathaniel Potter, M. D. Professor of the Practice of Physic; Elisha De Butts, M D. Professor of Chymistry; Samuel Baker, M. D. Professor of Materia Medica; William Gibson, M. D. Professor of Surgery; John B. Davidge, M. D. Professor of Obstetrics, Adjunct Richard W. Hall, M. D.

At a commencement held on the first Monday in May, the degree of Doctor of Physic was conferred on the following gentlemen, who published and defended their respective Dissertations, viz:

John O'Connor, of Baltimore, on Carcinoma or Cancer.

Charles L. Snyder, of Hampshire county (Virginia,) on Catamenia.

Henry Curtis, of Richmond (Virginia,) on the seat of Animal Life.

Corbin Amos, of Harford county (Maryland,) on Dysentery.

Thomas D. Jones, of Somerset county (Maryland) ——.

The courses of Lectures during the next session of the Classes, will be delivered in the new College, in Lombard street, as the progress of the building justifies the regents in the assurance that suitable apartments will be completed previous to the first of November.

MEDICAL OBITUARY.

At New York, Edward Miller, M. D. physician of the port of New York, and one of the editors of the Medical Repository.

In Harford County, Md. Jacob Hall, M. D.

At George Town, Potomack, William Baker, M. D.

In Calvert County, Md. James Gray, M. D. and Edward Gantt, M. D.

The hurry in which the three first numbers of the Lycæum were printed, occasioned a number of errors, which the reader will please correct, as follows:

In page 8 line 31, for als read sal.—same page, last line, for me read some.—p 10 l 7, for reent read cast:—p 11 l 10, for relapses read relapse:—p 12 l 16, for enforce read inforce:—l 26, for tremors read tumors:—p 24 l 5, from bottom, for gongrene read gangrene:—p 26 l 16, for coloric read caloric:—p 36 l 26, for exacertation read exacerbation.—p 43 l 23, for epilottis read epiglottis:—p 55 l 25, for collosum read callosum:—p 63 l 4, for peduncali read pedunculi:—p 66 l 5, for requetly read frequently:—p 67 l 6 for ainmals read animals: p 83 l 10 for abervations read aberations:—p 84 last line for atatck read attack:—p 85 l 7, for voses read vocis:—p 89 l 12, from bottom for aa read as:—p 93 l 4, for color read colour.—p 96 l 14, for premordia read primordia:—p 106 l 16, for asme read same:—p 109 l 33, for ignated read ignited:—p 158 l 32, for then read than:—p 176 l 3, for then read than:—l 9 for caitilaginious read cartilaginous:—p 210 l 1, for docomposition read decomposition:—p 216 l 18, for theorectial read theoretical:—p 220 l 11, for prportion read proportion:—p 243 l 28, for modeate read moderate:—p 253 l 9, from bottom, for adhersive read adhesive:—p 257 l 2, from bottom, for high read neighboring:—l 17, from top, for unequivical read unequivocal: p 259 l 4, from bottom for appa read appomatox:—and in l 2, from bottom, for Fridericks read Fredericksburgh:—p 260 l 10, for great read greater:—p 261 l 7, for general read personal:—l 21, for fertalige read fertilize:—p 263 l 8, for granits read garnets:—p 264 l 11, for viridescent read iridescent:—p 265 l 4, of the note for by the Mr. read by Mr.—p 267 l 22, for grangue read gangue:—p 269 l 5, from bottom, for pollucid read pellucid:—p 270 l 10 and 14, for emethyst read amethyst.

INDEX TO VOLUME I.

PAGE.
Prospectus, - - - - - 1
Williamson on Scrofula, - - - - 7
Home on the Structure and Uses of the Spleen, - 17
Dallam on the Use of Cotton in Burns, - - 22
Wright on the Epidemic Fever, &c. - - 26
Hopkin's Case of a Shot in the Trachea, - - 42
Report on Gall's Anatomy of the Brain, - - 45
Potter on the Anomalous Character of the Measles of 1808, 83
Review of Moore on the White Oxyde of Bismuth, 89
Drake's Notices of Cincinnati, - - - 97
Professor Davy's Electrochemical Researches on various Objects, - - - - - 101
Lehman on the Chymical Phenomena of Respiration, 151
Hall on the Influenza of 1807, - - - 155
Williamson on the connexion between Scrofula and Phthysis Pulmonalis, - - - - - 161
Artificial Dilatation of the Female Urethra, &c. - 169
Haslam on Tetanus in Horses, - - - 175
Wilkins on the Prophylaxis of Hydrophobia, - 179
College of Medicine of Maryland, - - - 195
Licentiates by the Medical Board, June, 1811, - ib.
Adjustment of professorships in the University of Newyork, ib.
Tribute of respect to the Memory of Fourcroy, - 197
Professor Davy's Researches on the Oxymuriatic Acid, &c. 201
Harsenepe's case of Canine Madness, - - 224
Williamson on the connexion between Scrofula and Phthysis Pulmonalis, - - - - 229
Sanford on Peruvian Bark, - - - 236
Haslam on the use of Cotton in Burns, - - 253
Hayden's Geological Sketch of Baltimore and its vicinity, 255
Saunders on Gum Lac, - - - - 271
Potter's case of Luxated Humerus, - - 275
Wright on the use of the Prussiate of Iron in Uterine Hemorrhage, - - - - - 279
Hermbstaedt's Experiment to obtain Sugar, - 284
Dorsey's case of Inguinal Anuerism, - - 292
Medical Law of Maryland, - - - 298
Bell's Anatomy of the Brain, - - - 303
Sanford's additional observations on the Peruvian Bark, 319
Cure of Syphylis by Alkalies, - - - 325
Williamson on Scrofula—in continuation, - - 329
Legallois on the principle of Action of the Heart and the seat of that principle, - - - - - 342

INDEX.

PAGE
Hints on the subject of Animal Secretions, by Everard Home, esq. F.R.S. - - - - 354
Kelso on the use of Cotton in Burns, - - 360
Urtica Whitlawi, - - - - - 361
Hopkins on the Poisonous effects of Strammonium, - 366
Observations on Albumen and other Animal Fluids, with Remarks on their Analysis by electrochymical decomposition, by Willam Brande, F. R. S. - - - 368
Magendie on the Organs of Absorption, - - 378
Hall on the use of the Acitate of Lead in Intermittents. 388
Papers on the Stone, - - - - 390
Davidge's Review of Mease's case of Varicose Aneurism, 406
Harvard University Prize Questions, - - 413
Discovery of Molybdæna in the United States, - 414
College of Medicine of Maryland, - - ib.
Medical Obituary, - - - - - - 416

END OF VOL. I.

[illegible] 279